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PHYSIOLOGICAL EXPERIMENTS AND OBSERVATIONS *on the cessation of the contractility of the Heart and Muscles in the Asphyxia of Warm-blooded animals.** By JAMES PHILLIPS KAY, M.D. House Physician to the Royal Infirmary, Edinburgh, and late Senior President of the Royal Medical Society. No. 1.

Respiration can be suspended for a very short space of time ere alarming symptoms supervene. Vertigo, with a sense of cerebral oppression, are premonitory. To these succeeds an internal agony of the chest, gradually aggravated, until, in cases in which the experiment is voluntarily made, it is arrested by the inward conflict which subdues the most resolute. The surface at a somewhat later period becomes livid; the cerebral faculties, and motion and sensation are abolished; and the organic heat gradually fails first in the extremities, afterwards in organs where the circulation is more vigorous, and there is less exposure to the atmospheric temperature. To a superficial observer all the functions seem to have expired from the moment in which the individual lost the faculties of sensation, motion, and his intellectual consciousness; but the vulgar are deceived by appearances in regarding this state to be actually death. The functions of the various organs may often, after this state has been for some time established, be restored to their natural and healthy standard. The contractility of the muscles and the action of the heart continue vigorous during a considerable time; and in other structures the apparent death is not real. Though the action of many organs has been changed into one which is incapable of giving evidence of life by its usual phenomena, an action continues which is capable of supporting a life peculiar in its phenomena, until, by the use of means to restore the function of the lung and circulating system, the natural actions are resumed. In a similar way a depression of the skull may suspend voluntary motion and sensation.—When the depression is raised they are re-

stored. The motive power, however, still continued in the muscles, but was not exercised from the want of a volition; and consequently no evidence of the surviving motility was evinced, unless the nervous influence had been excited by galvanism.

In the same way we may conceive that motions might be excited in the nerves of sensation by external stimuli, which would have so affected the cerebral organs as to have produced sensation, had not the action induced by these objects in the brain been so morbidly changed, or so feeble, as not to have affected the mind, so as to cause the phenomenon which we term sensation.

We can conceive no change to take place in any organ by which its function is supported without a motion; but whether this happen in simple fibres, or in secreting vessels, or in organic molecules, it is not necessary at present to inquire. That motion which supports the most healthy condition of the organ must be the best, and therefore there can be but **one** such action.

Every other must be less capable of supporting the function; and in proportion as they differ from this they will tend to suspend it. They become morbid; and we may conceive them to become so much so as altogether to change the result of the organization, and thus to pervert the function of the part. This we may conceive concerning every tissue in the body. The phenomena of all may be thus changed; and though it is improbable that organs, the accordance of whose action supported a life of certain phenomena, would be capable of assuming different actions, and yet preserve again the balance of their activity, so as to act together for any great length of time in the support of a life of different phenomena, yet we may conceive this to happen for a considerable period.

In this inquiry, therefore, we shall examine the minute detail of the phenomena, in order to acquaint ourselves with the manner in which each individual organ is affected. In this morbid state we shall find actions to continue for a very considerable period in all; in some, beyond the period in which any instance of the resuscitation of natural phenomena has occurred. Asphyxia appears to be a mode of life which has few of its usual phenomena, but

* Read lately before the Royal Medical Society, and communicated by Dr. Alison, Con-junct Professor of Institutes of Medicine, to the Medico-Chirurgical Society of Edinburgh.

in which actions proceed during a certain period in all the organs, which, by peculiar methods of treatment, are susceptible of being changed into those which exhibit the usual vital phenomena.

Death appears to us to consist in the disunion of the intellectual principle from the corporeal organization.

This connexion is maintained by various peculiar intestine motions in the various organs, of which one is the most perfect, and others are in various degrees morbid. General arguments appear to indicate the brain to be the intimate residence of this mysterious tenant. The intestine actions of the cerebral organization may be primarily or secondarily affected, that is, by the previous lesion of other organs, or without that lesion; but until these organic motions become such as to disunite the immaterial intelligence, life continues, though none of the usual evidence be given of its existence by organic motions. But as we cannot conceive, that when once the union between mind and matter has ceased, the intellectual principle can be recalled to the corporeal organization, all states in which resuscitation is said to take place must be only apparent, and consist only in a change from those morbid actions which suspended the usual phenomena of life, to those natural organic motions by which its usual functions and phenomena are supported.

Asphyxia differs from death, therefore, as in it the minute actions of tissues continue such as to support the connexion with the intellectual principle. Although in death, as after decapitation, (when we cannot conceive the intellectual principle to be connected with the corporeal organization,) contractility and nervous motion long survive, and minute organic movements are vigorous, yet they have lost that portion of the organization which maintains the union with the intellectual principle. Whilst life yet remains the minute actions may be so morbid that no means can revive those by which life can be supported for any period, and then death is certain. But the separation of the intellectual principle from the corporeal organization, may take place long before all motion has ceased, and then death has ensued; yet, as life may be restored in the early stages of asphyxia, it follows that death has not happened, and that this state must be regarded as life peculiar in its phenomena.

The first effect of interrupted respiration is to impede the progress of blood through the minute vessels of the pulmonary tissue. In the rapidity of its circulation there is a marked difference even in inspiration and expiration. Much might here be said concerning the pulmonary circulation in the different mechanical states of the lung, proving that it is retarded when the lung is comparatively collapsed, and *vice versa*; but the disquisition seemed to encumber this inquiry, and it has therefore, by the suggestions of a learned critic, been omitted.

When the access of atmospheric air to the lung is in any way prevented, a short interval

elapses; the animal makes an attempt at inspiration, resembling a deep sigh, which is necessarily ineffectual. This is followed by rapid gasping, by which the whole body is agitated, and every muscle which has power in respiration is most violently convulsed to overcome the obstacle to the admission of air. The surface and mucous membranes become livid, the sensorial faculties are abolished, and at the same moment the action of the voluntary muscles ceases. The circulation and the action of the heart continue for a short period only. On more minute examination the whole capillary system is found to contain only dark fluid blood; the left auricle a considerable quantity; the left ventricle scarcely more than is sufficient to moisten its parietes: the arteries are empty, but the venous system, especially where it empties itself into the right heart, is gorged. The right ventricle, and auricle, and pulmonary artery, are distended to the utmost with a dark fluid blood. If the asphyxia have been produced gradually, the colour of the blood is darker, and the engorgement of the lung more perfect; and this always differs in proportion to the length of time in which it has occurred; but the left ventricle and arteries are invariably empty. On this subject the opinions of Haller, Kite,* (p. 56.) Coleman,† Bichat,‡ Orfila,§ agree in the general fact, differing somewhat in particular points.

This state of the circulation proves that the principal obstacle to the progress of the blood must be sought in the lung. Bichat, whose opinions on asphyxia led him to attribute much to the influence of the dark blood upon the heart, assents fully in the truth of this conclusion.||

Besides the mechanical effects of air upon the lung, it has well known chemical relations with the blood, which must influence powerfully its progress in the pulmonary tissue. The minute vessels of all structures have peculiar sensibilities of organization, probably from the difference of their innervation, by which, in a healthy state, they circulate fluids of certain qualities only, and resist the introduction of any fluid foreign to their tissue by mechanical violence, so long as its peculiar sensibilities continue active. Thus, contiguous tissues circulate in their minute vessels fluids of very different qualities without commixture, and some in different degrees of the same succession of vessels always contain different fluids. In the lungs minute vessels exist, which convey the venous blood to the ultimate membrane of the pulmonary cells, where it is exposed to the atmospheric air. Changes ensue in its transition through this space which alter its qualities. The dark venous character is lost; it assumes the arterial red. It has more oxygen, and less carbon proportionally; and there-

* Elementa Phys. Vol. iii. p. 249.

† Suspended Respiration, p. 6 and 7.

‡ Sur la Vie et la Morb. p. 220.

§ Dict. de Med. Asphyxie, T. iii. p. 60.

|| Sur la Vie, &c. p. 227.

fore, *a priori*, we should be induced to conclude, that the innervation or organic sensibilities of the minute vessels which circulate the arterial blood, must be different from those which convey the venous. Reasoning from the general laws of the economy, it must have been regarded singular, had the vessels of arterial blood in the pulmonary tissue been capable of conveying blood, with equal velocity, in all stages of the venous degeneration. In suspended respiration we should expect, that, so long as the oxygen contained in the pulmonary cells should remain unexhausted, the ordinary changes of the blood would ensue, and the circulation for a period would continue with its usual vigour; and in proportion afterwards, as the oxygen diminished, the blood not undergoing that change which would be necessary, in order that it might permeate the vessels of arterial blood in the pulmonary tissue, that its circulation would be impeded. As the change, however, is only gradual, and the susceptibilities of action in all tissues are great, the venous blood would doubtless penetrate with diminished velocity the pulmonary organization, until its characters should be so greatly altered, as no longer to be capable of exciting the action of the vessels of arterial blood. If, then, air were admitted, it would re-oxygenate the blood exposed to its action in the pulmonary cells, and bestow on it those qualities which are necessary to its circulation.

An experiment of Bichat is beautifully consonant with these conceptions.* "If a stopcock be adapted to the trachea, exposed and divided, and afterwards it be shut, the blood becomes darker, and springs for some time with its usual force" from a divided artery, "but at length the jet becomes gradually feebler. Readmit the air, the blood becomes almost immediately red, its saltus also rapidly increases." The blood, it will be remarked in this experiment, was observed to flow from an artery dark, and must therefore have permeated the lung without undergoing the usual changes. This, however, continues for a very short period.

Exp. 1.—The trachea of a large rabbit was tied, the abdomen and chest opened, and at the end of the second minute from the commencement of the experiment, the external iliac artery was divided, a considerable quantity of dark blood flowed, but at the third minute it had almost ceased to escape. The heart continued contracting vigorously; very small quantities of dark blood collected slowly every twenty seconds at the extremity of the artery. In five minutes all flow of blood had entirely ceased. The left heart contracted spontaneously for a very considerable period longer.

I repeated this experiment with similar results. In experiments of this nature I felt aware that the cessation of the arterial action might be attributed to the quantity of blood, which escaped when the artery is divided, so very soon after asphyxia has been produced.

Exp. 2.—I therefore tied the trachea of a small hare. The chest was opened. During three minutes the blood issued *per saltum* with a gradually diminished force from the mammary arteries, and then ceased. On putting my finger on the aorta I could discover no pulse. The aorta was divided, and a very small quantity of blood flowed, and immediately ceased. After three minutes and a half had expired, no blood flowed from the aorta; little blood had been lost. The heart continued vigorous the usual period, and all the other circumstances of asphyxia were observed as usual.

This experiment was confirmed by subsequent observations. I however varied it thus:

Exp. 3.—A rabbit was asphyxiated by tying the trachea. The chest was opened. At the end of three minutes and a half no pulse could be discovered in the aorta. The left venous sinus was then opened, the blood contained escaped, and blood from a period of from one to two or three minutes occasionally collected in very minute quantities, as though it gradually drained from the larger vessels of the lungs, but never, as often as the experiment was repeated, collected in quantity. The heart continued vigorous the usual period, and all other circumstances of asphyxia were observed.

The supply of blood from the lungs to the heart is arrested, therefore, a very short period after asphyxia has been produced. But in the experiment of Bichat, when the flow of blood from a divided artery in asphyxia had ceased, by permitting the usual supply of air to the lung, the circulation was immediately re-established with its usual vigour. Without the supply of oxygen the blood undergoes the asphyxial degeneration; and the circulation being arrested, we must conceive that the vessels destined for the reception of arterial blood are incapable of conveying blood which has undergone this morbid change. But when oxygen is again supplied to the pulmonary cells, the blood becomes arterialized, and at the same moment the circulation is re-established, the minute vessels acting as usual, when stimulated by arterial blood. The celebrated experiment of Hooke is of a similar nature with that of Bichat. It consists in reviving animals apparently dead from asphyxia, by insufflating air into the lungs, until the circulation and natural respiratory motions are restored.* Haller, in whose day the connexion of chemical laws with animal physiology was little understood, attempted to explain these phenomena, simply by the mechanical effect of the air upon the lung. "*Sed pulmo per suas causas immeabilis, sanguinem ad ventriculum cordis sinistrum nullum trans mittebat. Is idem pulmo, per aerem vi impulsus, extensis vasis, diductis vesiculis, sanguini pervius redditur, ut ex dextro corde, ad sinistrum venire possit.*"

I endeavoured to ascertain the relation of

* Sur la Vie, &c. p. 222.

* El. Phys. Vol. iii. p. 250.

arterial and asphyxiated blood to the lung in the following manner:

Exp. 4.—Two rabbits being secured, the trachea of the first was tied, and the chest immediately opened. About the third minute in general no pulse could be discovered in the aorta. The left venous sinus was divided, and the blood contained escaped. Small quantities collected to the fifth minute, or even the seventh. The lungs never collapsed. The pulmonary artery being tied close to the root, the beak of the syringe was introduced into its trunk. The vena cava of the same rabbit was divided, and asphyxial blood received into the syringe and injected into the pulmonary artery. I was once or twice deceived in the result. When not injected in quantity and with force, it did not permeate the lung, though immediately afterwards arterial blood obtained from the aorta of the second rabbit, penetrated it easily, and was discharged in quantity. But on repeating the experiment, using large quantities of blood, and injecting with greater force, the asphyxial blood permeated, and was discharged at the opening made in the venous sinus. Though this experiment cannot therefore be adduced in favour of the opinion which has been entertained, concerning the chemical relations of the blood to the healthy action of the minute vessels of the pulmonary tissue and its circulation, yet it cannot be adduced against it, the power of operating being so very different, the strength of the human arm, with the mechanical aid of a syringe, containing a much greater quantity of blood, being contrasted with the power of the right heart of a rabbit, acting with little energy in comparison, and on an extremely small quantity of blood.

This part of the subject is much illustrated by the experiments of Nysten. Bichat had supposed that air injected into the veins of animals was fatal by its noxious influence on the brain; and, carried onward by the impetuosity of his sanguine mind, he fancied that he had seen spumous blood filling the carotids and arterial system.

His experiments were carefully repeated by Nysten* with a totally opposite result. No air was ever found in the left heart or arterial system; but the right heart was always distended with a spumous bloody fluid, or with air. Nysten concludes from this appearance, that the injection of air into the veins is fatal, by producing distention of the heart, and mechanically resisting its contraction. He supports this opinion by experiments, showing, that when the spumous fluid is evacuated from the heart, by large openings made in the veins of the neck, the circulation is restored. I think, however, his own experiments contain the evidence, upon which a different explication must be founded. When air is injected in small quantities, death does not ensue; and, allowing some interval of time to elapse between each injection, he found it possible to

convey into the circulating system of animals, without immediate death, much larger quantities than would produce that catastrophe, were the whole administered at one time. Two circumstances of great importance, however, succeeded those experiments. After repeated injections of air into the veins, the arterial blood assumed a darker hue. This effect in some measure depends on the absorption of a greater quantity of azote than oxygen in the dissolution of the air; but much must also be attributed to the embarrassment of the respiratory function, from the air present in the pulmonary blood-vessels.

When, however, repeated injections of air were performed at intervals, death did not ensue immediately. The injections were attended by some rapid and short respiratory motions indicating the pulmonary obstruction. These symptoms seemed in a great measure to vanish, when shortly after the animal coughed, which was the first symptom of fatal pneumonia, which in every instance rapidly terminated its life. These circumstances all tend to prove that repeated moderate injections of air into the venous system are always fatal, by occasioning great obstruction to the pulmonary circulation, and consequent inflammation. When exceedingly small quantities are injected, and the animal is left without a repetition of the experiment to the resources of his system, a temporary embarrassment ensues, and the animal gradually recovers. Nysten supposes that the gases have then been dissolved in the blood, and either been exhaled from the lungs, or passed into the general circulation. But when repeated injections are attended with a fatal pneumonia, he supposes, that, all the gases not being dissolved, the spumous fluid obstructs some of the pulmonary vessels, and excites inflammation. It seems therefore strange that he should not have regarded the lungs as that part of the system in which the obstacle to the circulation arose, when large quantities of air were injected. This mode of death is thus reduced to asphyxia, where the circulation, being arrested in the lungs, exactly similar phenomena ensue, from the interruption of the supply of blood to the various tissues. Manifestly, if small quantities of air have such fatal effects upon the pulmonary organs, we cannot conceive that much larger quantities would be able to traverse them.

The organic sensibilities of the pulmonary vessels appear also to be illustrated by the foetal circulation.

If respiration were not necessary to the transition of blood through the pulmonary tissue, it would be difficult to conceive why the *foramen ovale* and *ductus arteriosus* should form separate, less perfect, and unnatural channels for the circulation. Why must all the contingencies attending the dilatation of the lung be hazarded? Why, if there be no obstruction during the absence of the air from the pulmonary cells, if the blood might circulate as freely in the foetal as in the adult lungs, is it not transmitted through that medium, which, since it is longest employed, must be the best? In

* Recherches de Physiologie et de Chimie Pathologiques.

the adult, however, the deprivation of air is followed by a sudden arrest of the circulation. When oxygen is again supplied to the pulmonary cells, the pulse and life return. These phenomena indicate surely that the circulation is arrested in the lungs.

Goodwyn supposed that the action of the lung continued a longer period than the contractile power of the left heart; and that in asphyxia the circulation was interrupted, because the blood, permeating the capillary system and the lungs, became at length so far disoxygenated, that it was incapable of stimulating the left auricle, and stagnated in its cavity. We have however seen, that the action of the left heart continues long after blood has ceased to flow from a puncture made in the pulmonary veins. The experiments of Bichat also prove that the heart circulates dark blood for a short period. He injected dark blood into the left cavities of the heart without destroying their contractility, whilst respiration proceeded with the right lung, and the heart contracted vigorously.* "I† have also re-established," he says, "the contractions of the heart, weakened in different violent deaths, by the contact of the dark blood injected into the left ventricle and auricle from a syringe adapted to one of the pulmonary veins." It cannot, therefore, destroy the contractility. The phenomena of the cessation of motion in the left heart in common asphyxia are these. A smaller quantity of blood is received into its cavities, and expelled for a time vigorously into the arteries, the ventricle meanwhile diminishes in size, as the quantity of blood supplied becomes less, until at length, though spontaneous contractions still occur in its fibres, no blood issues from a divided artery, and the ventricle, by contraction, has obliterated its cavity. After this, blood slowly accumulates in the auricle from the large vessels of the lungs, and its contractility continues for a very considerable period. The obliteration of the cavity of the ventricle is especially evident in frogs and cold-blooded animals, where it becomes transparent. The contractility of the auricle, it must be remarked, continues, though it contains the blood which the hypothesis of Goodwyn supposes to be fatal to it. By reference to experiments in a more advanced part of this inquiry, it will be found, that the spontaneous contractions and motive power of the different parts of the heart continue, always in proportion longer as they contain more of the asphyxial blood. Thus, in the experiment of Haller, the left heart may be made to contract longer than the right, by retaining the dark blood in its cavity, and by discharging it by an incision made in the *venæ cavæ* from the right. The action of the auricle may even be restored to its former vigour, by re-establishing the circulation through the lungs, as in the experiment of Hooke, though evidently the insufflation of air into the lungs could not reoxygenate the dark blood contained in the auricle.

Bichat, however, observing that the circulation of dark blood continues for a period, and reasoning from thence that it must pervade all the tissues of the body, conceived that the functions of the various organs were destroyed, by the positive noxious qualities of the blood which circulated in their vessels. This theory, however, as far as it concerns the heart, is supported by no direct evidence. He relates no experiment by which the noxious influence of the dark blood on the heart is demonstrated. The theory principally rests on general arguments drawn from his experiments on other organs of the body. No analogy can, however, be drawn between the heart and other organs, excepting the muscles. The brain, upon which Bichat's most remarkable experiments were performed, has so little analogy with muscle, that, even were it allowed that his experiments conclusively prove the noxious influence of the dark blood upon the tissue of the brain, it would be impossible to infer that its influence on the muscles must be similar. The only direct evidence which Bichat adduces in support of his opinion, is the supposed influence of the dark blood on the muscles of voluntary motion.* "Inject into the crural artery of an animal this species of blood obtained from one of its veins, you will perceive the muscles soon becoming very sensibly feebler, and that sometimes immediate paralysis ensues." He then adds, that the ligature of the artery paralyzes the limbs, especially if it be placed upon the aorta; but does not attempt to show that the paralysis occurs earlier when dark blood is injected than when the artery is simply tied. The following experiment, however, proves that the effect is entirely owing to the ligature of the artery.

Exp. 5.—Three rabbits were secured. The abdomen of each was opened. The aorta of one was tied above the renal arteries, and also the left common iliac close to the aorta. In eight minutes the vena cava of the second was opened; the blood which immediately flowed was injected into the aorta of the first. The contractility of both extremities was found to be equally vigorous. At the fourteenth minute, the degree of contractility having been ascertained by the insertion of needles attached to the wires of a battery, another quantity of blood was injected. No difference could, by careful examination, be discovered in the contractility of the muscles in the two extremities. The injection of venous blood obtained from the cava of the third rabbit, was twice more repeated in the course of thirty minutes. At each interval between the injections, the wires were equally applied to the muscles of both extremities, their contractility gradually and equally declined in each, as in experiments in which the artery is simply tied. Some feeble contractile power survived one hour.—Wishing, however, to compare the effect in the muscles, and with the blood of the same

* Op. Cit. p. 215. † Ib. p. 216.

* Op. Cit. p. 278.

animal, *cæteris paribus*, the following experiment was performed. Present Dr. N.

Exp. 6.—If the blood of asphyxia have a more hurtful influence on the functions of muscle than the defect of the arterial blood, if we totally cut off the supply of blood to any muscle in asphyxia, its contractility ought to continue longer than all the others which are subjected to the influence of this fluid. The trachea of a rabbit was laid bare, a ligature was passed beneath it, and one also beneath the abdominal aorta, above the superior mesenteric artery. The aorta and trachea were tied nearly at the same moment. The muscles continued equally vigorous for some time on the application of the wires of the battery. They then equally and gradually declined in strength, and in one hour ceased, contracting to the stimulus at the same time, the muscle of the upper extremity appearing towards the close of the experiment to be somewhat stronger. This experiment was repeated. I afterwards varied it thus:

Exp. 7.—The trachea of one rabbit was tied. In another the chest was opened, and in one minute the heart tied at its base. The muscles continued contractile an equal time in both.

The supply of dark blood from the left heart in asphyxia has therefore no positive noxious influence on the muscles; but they are affected in exactly the same degree as when the supply of blood to their tissue is interrupted.

The connexion between the blood and the contractility of muscular fibre is most intimately involved in this inquiry. Blood is necessary to muscular contraction. The most superficial observer is at once impressed with the extreme vascularity and the deep red colour of its tissue when contrasted with that of the rest of the body. Its power is in the direct ratio of its vascularity. All diseases which diminish the quantity of blood impair its power, and most of the causes which augment it, are accompanied with a very evident increase of vascular energy. The colour in the young is scarlet, in manhood of a deeper red, and in old age it becomes a yellow and duller red.* It is influenced by sex, temperament, and diseases; is diminished in dropsy, consumption, hæmorrhagy, and in paralysis, the muscles often become pale and discoloured like cellular tissue. Bichat notices† that in many diseases of great prostration of strength the muscles become pallid; motion increases vascularity and power. "Some birds, as the black game,‡ thus have the external pectoral muscles of a deep colour, whilst the interior are pale." This appearance is observed in the most actively employed muscles of men of various trades. Fowler remarked§ that the contractions of a frog's leg were rendered

much more vivid by exciting the vascular action.

Boerhaave, Stenoh, Cowper, and many other of the older authors, knew that a ligature of the artery destroyed the contractile power of the muscle. Legallois, whose experiments in pursuit of other objects often led him to observe the same fact, conceived it merely to prove how necessary the circulation was to the nervous power. Etchepare* has lately more minutely observed its consequences. It must however be premised, that it is impossible to isolate any portion of the body by the ligature of large trunks, since the circulation in the capillary system continues, and with such vigour as to be able to supply nutrition to the part, until, by the increased size and activity of its vessels, it has again connected itself directly with the heart's influence. On ligature of the abdominal aorta in a dog, excessive weakness of the lower extremities ensued, and in eight or ten minutes the motions became almost imperceptible. I have witnessed these phenomena. The contractility is however perceptible by the stimulus of a powerful battery, from forty minutes to a somewhat longer period than one hour, though of course it continues but a very short time vigorous, and towards the close of the experiment the contractions are almost imperceptible.

It is remarkable, however, that though the presence of venous blood in the tissue of muscle, has been considered by Bichat and others to be fatal to its contractility, when the vein only is tied, or the artery and vein together, preventing the egress of this blood, the phenomenon does not so soon cease as in the simple ligature of the artery. The power of muscle does not become instantly extinct on the ligature of an arterial trunk, I imagine, because its capillary system still contains a great quantity, and the ligature only prevents the supply of blood from the arterial trunks which irrigate this system, whilst the communication with the whole capillary system, which contains a much greater quantity of blood than the arterial, remains unobstructed. In purpura and scurvy the blood does not coagulate after death, and these diseases are marked by great prostration of strength. In animals which have not the power of quick motion,† the arteries supplying muscles are subdivided into a net-work of extremely numerous small branches, which again unite, retarding the progress of the blood by this mechanical arrangement. From these facts Sir A. Carlisle concludes, that, for the rapid and frequent motions of any muscle, it is necessary that it should enjoy an ample and unobstructed supply of blood. Frequently also in diseases of the arteries, muscular debility, or from their obstruction, paralysis ensue. The subject is so extensive, however, that it is impossible to give a detailed view of it in this place, and I

* Dic. des Sciences Medical, T. xxxiv. p. 562.

† Anat. Gen. T. iii. p. 333.

‡ Phil. Trans. Carlisle, C. L. 1805.

§ Animal Electricity, p. 128.

* Magendie's Journal de Phys.

† Home's Comp. Anat. Carlisle, Croonian Lecture, 1800, 1805.

shall refer to the sources of information.* I have also in my possession the aorta and obliterated femoral artery of a case of this nature, in which the paralysis was perfect.† The power of contraction is therefore in the direct ratio of the quantity of blood circulating in its tissue, and its capacity is determined by the facility and velocity of that supply. When it is abstracted contractility ceases. Blood is therefore necessary to contraction.

Having from the preceding reasoning formed this conclusion, I inquired whether, after the contractility of muscle had expired from the ligature of an artery, it could be revived by the transfusion of blood, and whether of arterial only or of venous also.

Exp. 8.—A large rabbit and two small ones were confined. The chest of the first was opened on the left side, the aorta divided, and the beak of the syringe introduced into the lower portion. After waiting some time, it was found by repeated application of the wires, that the contractility was extremely feeble in the lower extremities. The chests of the small rabbits were opened, and ere this could be performed the contractility of the inferior limbs was found to have expired. The aorta of the small rabbits were successively divided; the blood received into the syringe and injected into the aorta of the large rabbits. On applying the needles of the different poles of the battery to the muscles of the lower extremities, their contractility was found to have revived to the degree in which it was found after dividing the aorta of the animal. Present Dr. N.

Exp. 9.—In this experiment a large dog was secured. The arterial blood was obtained from his carotid, and the abdominal aorta tied. After the contractility had expired a considerably longer period than in the last experiment, the arterial blood was received into a warm syringe, and injected into the abdominal aorta whilst the animal was still breathing. The contractility of the muscles of the lower extremities revived. They contracted vividly a few times, and then became motionless. The blood of the dog coagulates so soon, that from this reason probably the power of motion expired earlier than in the other cases. Present Dr. N.

Exp. 10.—This experiment was performed on rabbits. The blood being taken from the abdominal aorta of one, and injected into that of the other, when the motive power was no longer perceptible, by repeated application of the wires of a galvanic battery in vivid excitation, the contractile power immediately revived, and continued vivid for some time. Similar experiments having been afterwards per-

formed with blood of a different character confirming this result, I did not repeat these experiments oftener, especially as to the most eager adventurer in physiology, there is much that is deeply repugnant to the feelings in its pursuits.

Arterial blood is more favourable to contraction than venous. Some physiologists have even asserted that it is essential. Buffon found that newly born animals have the faculty of living a longer time submersed than adults. Edwards, in a work of the most laborious investigation and important results, discovered that this is intimately connected with the faculty of generating heat,* young animals producing less than adults; and that the period during which life continues in asphyxia is intimately connected with this law in animals of the same and different species, and in animals of the same and different ages. Thus hibernating and cold-blooded animals, at certain seasons of the year, may be said to have no proper heat, but assume the temperature of the atmosphere. The faculties of generating heat are less than in animals of warm blood; and at the period when least heat is produced, in winter, they live submersed much longer than animals of warm blood. In young mammiferous animals the faculty of living without respiration is intimately connected with this law, and they may be divided into two classes: 1. Those which produce so little heat that they may be said to have no proper temperature, but are influenced by that of external agents. 2. Those which generate sufficient heat to preserve an elevated temperature when the air is not too cold.

The first live long in asphyxia, the others a shorter space of time. In the submersion of these young animals, however, the circulation proceeds; and as the lungs have not been dilated by respiration, it must resemble that of the *fœtus*, the *foramen ovale* and *ductus arteriosus* remaining open. Buffon even thought, that by repeated submersion these channels might be caused to remain pervious, and that thus the animals would preserve the faculty of living a long period without respiration. The phenomenon, however, (were the result of the experiment, as it respects the channel of fœtal circulation, successful,) could not ensue, as the faculty of generating heat increases with the age of the animal, and it requires a greater supply of oxygen. One just conclusion, however, Edwards has, I think, neglected to draw from his experiments. Inquiring whether the blood in this state tends to support life in mammiferous animals, he asphyxiated them by submersion, but arrested the circulation by an excision of the heart in some. Those, in which the circulation was arrested, lived only half the period during which the others survived.

The same laws apply to the cold-blooded animals. In summer, and when exposed to high temperatures, they produce generally a greater quantity of heat, and lose the faculty of living long without respiration. But when

* Trans. of a Society for the Improvement of Medical and Surgical Knowledge, Vol. iii. p. 448. The Medical and Surgical Journal, Vol. xv. p. 14 to 16. Phil. Trans. Vol. xxiii. p. 1194. Granger's Medical and Surgical Remarks, p. 182.

† Edinburgh Medical and Surgical Journal, Vol. xvi. p. 308.

* Sur l'Action des Agens sur la Vie.

their temperature declines under the influence of continued cold, their faculty of generating heat is diminished. I have repeated the experiments in which Edwards discovered that cold-blooded animals can live respiring only the air contained in water. When they have been long exposed to a very low temperature, and water is at zero, they live submersed in water without oxygen two or three days. Their lungs being only an appendage of the general system, the circulation is not obstructed in them when they do not breathe, but continues as before. But if, *cæteris paribus*, they be thus contrasted, if the circulation be suppressed in some by the excision of the heart and bulb of the aorta, and others be exposed to similar circumstances with no supply of oxygen, but without excision; those, in which the circulation continues, live thrice the period that those, in which it is suppressed, survive. He concludes justly, that the asphyxial blood in these animals tends to support life.

But in the young animals, where the *foramen ovale* and *ductus arteriosus* closed, the circulation could not proceed, since the lungs, when they have not been dilated by atmospheric air, are not permeated by the blood of the general system, and after respiratory motions have been established, the circulation is arrested immediately after the loss of the usual supply of oxygen. The life of these animals is prolonged, therefore, because the lungs in asphyxia do not form in them an obstacle to the circulation of the blood, though the period to which this life extends is increased by their low faculty of generating heat.

We have remarked, that in these instances asphyxial blood certainly tends to support life. Its relation to muscle must be now minutely examined.

In former experiments, we have observed that the ligature of the aorta produces paralysis in various periods of time, seldom or never exceeding forty minutes or one hour; but the ligature of a vein occasions paralysis after some hours only. I have often performed experiments, of which the following is an example.

Exp. 11.—In a full grown rabbit, the common iliac artery of one side, and the common iliac vein of the other, were tied.

A galvanic battery of forty plates was used to stimulate the muscles of the extremities, the integuments of which were removed from an equal space. The wires of the battery were applied every quarter of a minute to each extremity, for seven minutes, when the contractions became weak. In about a quarter of an hour they were imperceptible in the limb in which the artery was tied, and ceased shortly afterwards, probably because the battery was very powerful, and they were constantly stimulated, though both limbs were equally galvanized. The contractions in the other appeared to be under the influence of the will. The contractility continued, though somewhat feebler, but in three hours the change had not proceeded rapidly; the contractions were still energetic. The cessation of con-

tractile power ensues earlier when both the artery and vein are tied, but not so soon as when the artery alone.

Dark blood is therefore less favourable than arterial to the contractility of muscle, but its presence in the tissue supports this power a considerably longer period than when the artery is simply tied. That venous blood is favourable to muscular contraction is confirmed by the following experiments.

Exp. 12.—The abdomens of two rabbits were opened, and the abdominal aorta having been tied above the renal arteries, the beak of the syringe was introduced into the lower portion. The common iliac artery of the left side was then secured. The motive power continued strong to the stimulus of the battery half an hour. I then observed that the vena cava was extremely distended, and remembering M'Fadyen's experiments on the motion of the blood in the veins, I aided its ascent by constantly drawing my finger along the vessel towards the heart, and then preventing its return by pressure, and performing the same action with the other hand. After having continued this about three minutes, during which time a very considerable quantity of blood had been propelled towards the heart, I was surprised to find that the muscles, which had before been very energetic, had almost lost their contractility—the feeblest and most tremulous motions only being performed. Immediately afterwards, no contraction could be obtained from repeated applications of the wires. The vena cava of the second rabbit was divided, the blood received into the syringe, and injected into the aorta of the first. The contractile power of the right leg, in which the artery was pervious, immediately became strong; that of the left limb, in which the iliac artery was tied, remained extinct. In a few minutes more, a second quantity of blood, which had collected from the divided cava, was injected, and in a few minutes a third. Vivid contractions ensued, on the application of the stimulus in the right limb, and continued, though with decreasing force, a quarter of an hour.—Present Drs N. and B.

Exp. 13.—The experiment was repeated on two smaller rabbits. The aorta and left iliac of the first were tied. In half an hour the contractility of its inferior extremities became weak, and then suddenly expired. Care was taken by repeated applications of the needles attached to the wires, to prevent any motion remaining unobserved. A small quantity of blood only, from the size of the animal, could be obtained by the syringe from the cava of the other. It was injected, and the contractility of the right leg, in which the artery was pervious, immediately and remarkably revived on applying the stimulus; the other leg remained perfectly immotile. The injection was repeated with increased effect. The experiment could not, however, be prosecuted any further from the want of blood, and the motive power soon expired.—Present Drs. N. and B.

I had observed, that after the ligature of an

artery, the congestion of blood in the veins of the muscle maintains its contractility a longer period than when the artery is simply tied. I then inquired whether the asphyxial blood had the same power. In experiments, I had found that the asphyxial blood, when freely supplied by the natural contractions of the heart to the muscles of voluntary motion, had no positively hurtful influence. The contractile power of such muscles continued as long as in those to which the supply of this blood was prevented. But the congestion of venous blood appears to have the power of maintaining contractility a certain period. Does the blood of asphyxia maintain this phenomenon of muscles in a similar way? I observed that the muscles of the superior and inferior extremities were equally congested, from the accumulation of blood from the general system of minute vessels in all the structures of the organization, the inferior with venous, in the sixth experiment, and the superior with asphyxial blood, and the contractility remained during the same period in each. Was not, I inquired, the contractile power of the superior extremities maintained by the congestion of asphyxial blood in their veins?

Exp. 14.—The abdominal aorta and vena cava, and the trachea of a rabbit, were tied at the same time. The chest being opened, the lungs were found well dilated. After a few minutes had expired the superior cava was divided. The needles attached to the wires of the battery were then applied to the muscles of the upper and lower extremities; they were equally and vigorously contractile. From the superior cava flowed a great quantity of blood, which had the asphyxial characters. The muscles of the upper extremities became gradually feebler, and their contractility expired in thirty-five minutes. Those of the lower extremities were very vigorous, when the contractility of the superior was extinct, and continued so during twenty minutes afterwards; and contractions could be obtained by the application of the wires, at the expiration of one hour and a quarter.

These experiments appear to prove, that neither asphyxial nor venous blood have any noxious influence upon the contractility of the voluntary muscles, but that their presence supports the contractile power for a considerable period. The analogy which therefore Bichat has drawn from the influence of venous blood upon the voluntary muscles is futile.

The heart, it may be objected, has a peculiar sensibility; it differs from the voluntary muscles in having a peculiar innervation—in its constant action—somewhat in the distribution of its vessels—and the right auricle is regarded by physiologists as the *ultimum moriens*. These circumstances are sufficient to throw on any analogy drawn from the muscles of voluntary motion a doubt, which, in the investigation of truth, it is necessary to remove. The fact, that the right auricle continues longer motile than any other muscle, has been regarded since the days of Galen as evidence of some peculiarity in the contractile

power of the fibres of the heart. It has been generally esteemed to prove that the heart has, in the language of the day, a greater proportion of vital energy, or in that of Haller, a greater irritability.* For instance, Orfila,† commenting on the opinions of Bichat, says, “the heart which is not so soon paralyzed as the other muscles continues still some time to propel the venous blood to all the structures.” The continuance of the contractility of the right auricle, after all other muscles have ceased to act, appears to favour this opinion. Nysten and Haller‡ agree in their observations on this subject. The right ventricle and auricle in common death always contracting longer than their corresponding systemic cavities, and the venous sinus and auricle longer than the ventricle. In the experiments of Nysten the action of the ventricles expired before that of the voluntary muscles. The dependence of the muscular contractility on the proper supply of blood has been previously shown, and it is probable that the continuance of contractility, after the obstruction of the great vessels, depends on the general anastomoses of small branches, and the uninterrupted communication of the capillary system, which maintain a supply sufficient to support a feeble contraction. Now it is evident, that, from the imperfect connexions of the heart, with the general capillary system, by means of continuous tissue, this communication must be more imperfect in this than in any other organ; and that, consequently, its contractility, unless supported by some other means, ought first to expire. But the right auricle ceases last to contract. The contractility of different parts of the heart expires at different periods.

Have these phenomena any connexion with the state of its vascular system, or by what other means is the contractility of the right side of the heart supported longer than that of the left? The continuance of the contractility of each auricle beyond that of its ventricle may perhaps be attributed to the difference of tissue between an auricle and a ventricle; either to the actual difference of the muscular or of the nervous system in an auricle, which, exposed to the same causes, continues to act longer than a ventricle. But we find that the right auricle continues its contractions longer than the left, and the right ventricle than the left, and the reason of this difference is the object of the inquiry. Haller supposed that the motion of the heart was maintained for a longer period, because the natural stimulus which excites the motion of its cavities was present in them to prevent their quiescence. The heart, however, being a muscle, must be subjected to the general laws of muscular fibre. The muscle in life is fatigued by exertion, and after the ligature of an artery, or when the circulation of the blood has ceased, stimuli weaken the remaining force of the contractile power, which expires earlier in pro-

* El. Phys. p. 481, Vol. i.

† Dict. de Med. p. 75. Asphyxie.

‡ El. Phys. Tom. i. p. 423, 24, 25, 26.

portion as the iteration of the stimulus is more rapid, and as its power is greater. When the supply of blood to the coronary arteries has ceased, each successive application of stimuli must tend therefore to weaken the power of the heart's contraction. The deduction therefore from the following celebrated experiment of Haller must be different from that which he maintained. "I* divided the *venæ cavæ*, and emptied the right auricle and ventricle, afterwards tying the veins. I opened also the pulmonary artery by an ample wound, that the right ventricle might more easily discharge its blood, so that the right auricle and ventricle evacuated their contents; on the other hand, I left the pulmonary veins free, but placed a ligature on the aorta, so that the left auricle and ventricle received blood, but could not expel it. The left auricle and ventricle always continued acting longer than their corresponding cavities, even to the fourth hour. The right auricle was in perpetual quiescence. The right ventricle contracted either imperfectly, languidly, or a shorter time, when it was not altogether immoveable, or its quiescence was perfect, as often as the experiment had so succeeded, that the cavities were entirely evacuated.

From this experiment Haller infers, that the presence of the blood in the cavities of the heart maintains by its constant stimulus the contractility of the fibre. But the left auricle always contains a quantity of blood in common death, sufficient, in the language of this hypothesis, to act as a stimulus in maintaining its contractility; yet it always expires earlier than that of the right auricle.

The congestion of the venous blood in the muscular fibre maintains, during a certain period after the supply of blood has ceased, the contractile power of muscle. The heart, in the death of the brain and lungs, is similarly situated with a muscle whose artery is tied. Blood for a certain time is supplied from the pulmonary veins, and during a certain time its contractions continue as strong as they usually are. As this blood diminishes in quantity, the supply to the coronary arteries becomes less, and the contractions of the heart are impaired. After, however, all supply to the arteries has ceased, the heart continues to contract; but it rapidly becomes feebler, as it now depends for the maintenance of its contractility solely on the blood contained in its fibre. So far the heart obeys the law of muscular fibre in the intimate connexion of this phenomenon with the supply of blood. If then, by any means, the egress of blood from its veins were obstructed, this must tend to support its contractility in this state.

Vieussens first, and afterwards Thebesius, Lancisi, Lieutaud, and many other later authors, have described veins of moderate size which enter into the cavities of the right venous sinus and auricle, especially. Haller†

confirms their testimony, and describes one small vein whose mouth is generally discovered near the *fossa ovalis*, which returns blood from the right auricle and its sinus. Other similar terminations of conspicuous veins are found in the right sinus and auricle, and, according to Vieussens, even in the *vena cava*. Afterwards in a similar way he describes the venous terminations in the left auricle, its sinus, even in the pulmonary vein. Supported by the same authorities, Vieussens, Thebesius, Rochet, Verheyen, Lancisi, Kawe Boerhaave, &c. other smaller veins have been discovered opening into all the cavities of the heart, and into the sinuses of the *venæ cavæ* and pulmonary veins, with oblique mouths which are protected by valves. Haller informs us that if the tube of an injecting syringe be introduced into any vein of the heart, and all the mouths of the smaller veins that can be discovered opening into the cavities be tied, any coloured liquid or mercury may be injected easily, and will be observed to exude through the oblique terminations of the veins, in the auricles, sinuses and ventricles, but chiefly in the *septum cordis*, and in the right cavities. The *arteria profunda* of the *septum cordis* has no *vena sociæ*, and it is probable that some other way has been prepared by nature by which the blood of this artery may return to the general circulation; and Haller insists upon the necessity of its return to the cavities, by veins similar to those described. The deep-seated veins of the muscular fibres of the heart must, it is evident, have more direct communication with its cavities than the superficial, since, in passing through an interlaced tissue alternately contracted and relaxed, their circulation would be nearly obstructed, and the functions of that part of the tissue would suffer in proportion. In the experiment of Haller, quoted above, the permanence of contractility was transferred from the right to the left cavities, by emptying the former, and producing a congestion of blood in the latter. This congestion, which maintained the contractile power of the left cavities of the heart, must have impeded the return of blood by the small veins which enter them, and produced in the fibre that venous turgescence which, when the supply of blood is obstructed, supports contractility. In the cessation of the heart's action from that of the brain or lung, the left ventricle is found almost empty, and its contractility first expires; the right ventricle, which must be contrasted with it, is gorged, and its contractions continue a much longer period. When, however, their state is changed, and the left is gorged and the right empty, the phenomena are reversed. The left auricle also in this state contains a considerable quantity of blood, but the right sustains the whole pressure of the venous and capillary system, and its contractility is most permanent; when the circumstances are reversed, by emptying the right auricle, and maintaining a congestion of the left, the contractile power of the right expires first. Nysten also observed, that the contractility of the

* 492, El. Phys. Vol. i.

† El. Phys. Vol. i. p. 380.

pulmonary cavities was greatly impaired by injections of air into the veins; if the injections were excessive the phenomenon very rapidly ceased. A distention with air, or a spumous fluid, must operate not merely by preventing any congestion of the internal veins, and allowing a free escape of blood contained in the fibre, but, as in paralysis of the bladder from a similar cause, by preventing a supply of blood to its fibre. Smaller injections of air, which are not so soon fatal to the contractility of the pulmonary heart, must act simply in the former way.

The preceding arguments led to the following experiments.

The left auricle generally contracts longer than the right ventricle, according to Nysten. Has this any connexion with the blood contained in its cavity? To determine this point the following experiment was performed.

Exp. 15.—The chest of a rabbit was opened, and in a minute and a half from the commencement, the lungs had been tied at their base, the heart was contracting rapidly and vigorously. At the fifth minute its contractions occurred irregularly and slowly. At the eighth they were eighty-eight. Tenth—left ventricle much diminished; right gorged; contractions feeble. Twelfth—left auricle has ceased; contractions twenty-four. Fifteenth—contractions twenty; extremely feeble. Eighteenth—left ventricle immotile. Twenty-fifth—fourteen contractions of the right side. In thirty-five minutes the ventricle and auricle were also immotile to common stimuli. The right side was gorged, the left empty.

Exp. 16.—In a full-grown rabbit the trachea was tied, and in two minutes the chest had been opened, the lungs tied at their base, and the *venæ cavæ* divided at the distance of one-fourth of an inch from the right auricle. The auricle was evacuated; but as no pressure was made on the ventricle, and the pulmonary circulation was arrested, it remained distended. At the sixth minute the auricles were flaccid—their cavities appeared empty—their contraction had gradually become feebler, and less frequent; at the end of eight minutes they were immotile: the *sinus venosus* of the right auricle contracted, however, a somewhat longer period. The left ventricle gradually diminished in size; its contractions became feebler and less frequent; and at the end of eighteen minutes, when it appeared to have expelled all its blood, it became immotile to common stimuli. The right ventricle contracted vigorously at the end of half an hour, and continued longer. On examination, all the cavities, except the right ventricle, contained no more blood than was sufficient to moisten their parietes; but the right ventricle was distended.

Exp. 17.—The left side of the chest of a rabbit was opened, the pericardium cut, and a ligature having been passed round the aorta and pulmonary artery at their origin, an interval was allowed to elapse, during which the animal breathed well, though rapidly, with the right lung. The ligature was then tied close to the root of the arteries. Respiration

proceeded well with the right lung, and continued about a minute and a half, when struggles and violent convulsive gasping ensued, and the animal died exactly as in asphyxia. The struggles had ceased in three minutes and a half. The trachea was tied when all motion had ceased several minutes. The right cavity of the pleura was then opened. The mediastinum was discovered perfect; but in the interval which elapsed ere the trachea was tied, some air had permeated through it into the right pleura, and the lung had in some degree collapsed. At the close of the experiment the pulmonary artery was found gorged with blood, which flowed from an incision in very great quantity. The aorta was nearly empty. The circulation had therefore been arrested in its full career, and the cavities of the heart must be distended with blood. The contractions of the ventricles continued synchronous, and they both contracted the last time at the thirty-fifth minute. The auricles contracted one hour and thirty-five minutes synchronously. The right then contracted most frequently. In one hour and forty-two minutes the left ceased to contract spontaneously, though, doubtless, it was contractile to stimuli. The right auricle contracted spontaneously two hours and ten minutes, and was then contractile. The ventricles were equally distended with blood, and ceased contracting at the same time. The right auricle was more congested than the left, and continued longest contractile, sustaining, as usual, the pressure of the whole venous and capillary system.—Present, Dr. N.

The result of these experiments is perfectly consonant with the idea, that the contractile power of the parts is supported, through a comparatively longer period, from the repletion of their cavities, and consequent congestion of their fibres with venous blood. This repletion of the right auricle, and of its venous sinus in death, from the cessation of the functions of the brain or lungs, must be more considerable than that of any other muscle of the body. The blood, which by the last contraction of the ventricle is propelled into the arteries, is by them conveyed to the capillaries in the various tissues of the body. The organic motions of these tissues continue for some time, the blood still moves onwards, and is impelled with a certain force from their actions into the general venous system. The veins are everywhere found distended, because the progress is obstructed by the lungs, in which respiration has ceased. The pressure of this power must be divided between the terminations of the large veins in venous radiculæ in all the system, and their opposite terminations in the heart. The power, therefore, will be greater in the auricle than in any other portion of the body; and if congestion support the muscular power, that of the auricle (as experiment proves it in these circumstances to happen) ought to survive longer than any other.

In the following experiment, which is in other respects a repetition of that of Haller, the venous congestion was prevented from af-

fecting the right cavities of the heart, and the result was consonant with the previous reasoning.

Exp. 18.—The chest of a rabbit was opened, the *venæ cavæ* tied, and then divided nearer the heart than the ligatures. The heart contracted vividly for some time, then gradually weakened. In ten minutes spontaneous contractions seemed to have ceased. It continued contractile without irritation to the thirteenth minute. The right ventricle and auricle were immotile to the stimulus of the galvanic battery, at the fifteenth minute flaccid and empty. The stimuli were constantly applied, to ascertain the motive power of the different parts. At the twenty-fourth minute no contractions could be obtained from the left ventricle. At the twenty-eighth, the left *sinus venosus* was observed to contain a considerable quantity of blood, which had gradually accumulated from the large vessels of the lungs. The blood was evacuated by an incision, and speedily all contractility ceased. The voluntary muscles, however, continued vividly contractile at the end of one hour, when the experiment was interrupted.—Present, Dr. N.

These experiments are consonant with the idea, that the motive power of the right heart, in all cases in which it has been observed to continue longer than that of the muscles, has been preserved by the greater comparative supply of blood to its fibre, from the congestion of its cavities, and consequent repletion of its small veins.

It would be difficult to determine whether much of this effect may not be owing to imbibition, by the open mouths of the small veins, from the blood contained in the cavities of the heart, thus supplying, by their peculiar action, the muscular tissue with the blood necessary to its contraction. Some experiments which I have performed seem to favour this idea. They have not, however, been so frequently repeated as to deserve mention here.

From these experiments, we may at least conclude, that the heart so far obeys the laws common to muscle, that an analogy, drawn from the influence of blood on the tissue of the muscles of voluntary motion, would be strictly applicable to the laws of the contractility of this organ. The blood of asphyxia, so far, therefore, from having any positively noxious influence on the muscles of voluntary motion, or on the heart, is capable of supporting their motive power for a certain period. By what arguments, therefore, is the celebrated hypothesis of Bichat supported?

Bichat commences his inquiries on this subject, by noticing the theory of Goodwyn, (p. 211.) To this he objects, 1st, That if the dark blood paralysed the left heart by its presence in the ventricle, the action of the heart ought to cease first; and that of the organs to succeed, which is contrary to the fact. 2d, If the trachea of any animal be obstructed, (p. 213,) any artery being divided, the blood which flows gradually assumes a darker colour, and at length becomes as dark as the

venous. Notwithstanding this phenomenon, which is very conspicuous, the fluid continues still to jet forth, with an energy equal to that of the florid blood for some time. Therefore the heart acts vigorously with this blood in its cavity. 3d, The result of this experiment is the same, though the atmospheric air be in a great measure abstracted from the lung by means of a syringe. 4th, Venous blood injected into the left auricle and ventricle, by means of a syringe, does not weaken the contractions. He therefore inquires, (p. 217,) *quelles sont donc les causes qui interrompent la circulation dans le cœur à sang rouge, et dans les artères, lorsque le poumon y envoie du sang noir?* He concludes without any further argument or proof, (p. 217,) "I think that the dark blood acts upon the heart in the same way as on all the other structures, as we shall discover that it influences the brain, the voluntary muscles," (which I have shown to be incorrect,) "the membranes, &c. in a word, all the organs which it penetrates, that is by permeating their tissue, by enfeebling each particular fibre; so that I am well persuaded that if it were possible to propel by the coronary artery, dark blood, whilst the florid circulates as usual, in the aortic auricle and ventricle, the circulation would be almost as soon interrupted as when the blood penetrates the tissue of the heart by the coronary arteries, only after having passed the two cavities of arterial blood."

That the genius of Bichat erred in this conclusion is perhaps sufficiently demonstrated. We have before seen in experiment 3, &c., that the action of the left ventricle continues long enough to expel all the blood from its cavity, and that it contracts long after blood has ceased to flow from the coronary veins. Its contractions, therefore, survive the period when it is possible that the coronary arteries can receive the asphyxial blood. The theory of Bichat supposed the noxious qualities of this blood to be fatal to the heart, and that, from this cause, the arterial circulation ceased; but the heart in asphyxia continues to contract vividly when no blood flows from a divided artery. The circulation is arrested by the suspension of respiration, and the consequent cessation of the functions of the lung, at the moment when Bichat supposed the circulation of dark blood to exercise a fatal influence on the heart, which, notwithstanding, continues its contractions vividly for a considerably longer period. But, we have before shown, that the contractions of the heart become less frequent after the supply of blood to the coronary arteries has ceased, in the same degree as the motive power of voluntary muscles is affected after an artery is tied. Other experiments have shown that this blood is favourable to the contractility of the voluntary muscles, and that its presence in the cavities of the heart maintains, during a certain period, the motive power of its fibres. Similar evidences may be obtained, though somewhat more obscurely, from the experiments of Bichat. In opposing the theory of Goodwyn, (p. 216,) he says,

"Enfin il m'est également arrivé rétablir les contractions du cœur anéanties dans diverses morts violentes par le contact du sang noir injecté dans le ventricule et l'oreillette à sang rouge avec une seringue adaptée à l'une des veines pulmonaires," evidently by giving a new supply to the coronary arteries. In page 217, the comparison of two opposed passages appears unfavourable to his opinions. He asks, "Quelles sont donc les causes qui interrompent la circulation dans le cœur à sang rouge et dans les artères lorsque le poumon y envoie du sang noir?" Now, in page 227, he says, "Toujours par conséquent c'est dans le poumon que la circulation trouve son principal obstacle." The circulation, therefore, is not arrested in the heart; and since the obstruction is thus said to be in the lungs, the supply of dark blood to the heart cannot ensue, and one passage contradicts the other. Yet he concludes in the answer to the question quoted above, that the noxious qualities of this dark blood destroy the motive power of the heart.

An experiment is also detailed in page 217, which is irreconcilable with the theory. When the dark blood has flowed from a divided artery some time in asphyxia, its impetus gradually becomes weaker, and at length ceases almost entirely; if then the stop-cock obstructing the trachea be opened, and respiration be allowed "il se rétablit bientôt avec force." If in this experiment the circulation had ceased from the destruction of the motive power of the heart, on account of the noxious influence of the dark blood upon its fibres, the renewed circulation of the lungs on the admission of oxygen could have no sudden influence on the fibre whose contractility had been destroyed by a noxious fluid, which had gradually permeated and pervaded its tissue, yet the contractions are described to have been soon re-established with energy. This, however, may be readily conceived, since we have seen, that the contractions of the heart cease from the want of a due supply of blood, on account of the obstruction of the pulmonary circulation. When, therefore, as in this experiment, the motive power has almost ceased, if respiration be restored, the circulation through the lungs will be re-established, the supply of blood to the heart recommence, its fibre renew its contractions with vigour. The same arguments apply to the celebrated experiment by Hooke, and to the usual phenomena of resuscitation by the inflation of the lungs.

In the application of the same doctrine to explain the interruption of function in the other organs of the body, Bichat has also drawn similar conclusions from the same premises. Examining his argument in detail, we shall discover that it rests entirely on the supposed influence of the dark blood on the voluntary muscles, and the supposition, that this blood continues to circulate a considerable period, and penetrates all the organs of the body; (p. 265.) We have observed, however, that the arterial circulation ceases in a very short time after respiration has been suspended. Bichat principally supports his opinion

by the fact, that the tissues of the body become more livid than in ordinary death, and that dark blood oozes from them when they are divided.

Blood darker than the arterial certainly permeates the lungs, and is transmitted from the left ventricle during a short period to the various organs of the body. But the time during which this blood circulates when the respiration has been suddenly suspended is so short, that we cannot suppose that it has any great influence in producing this colour, especially as the phenomenon is capable of an explication more consonant with the facts detailed in this inquiry. In the variety of asphyxia often observed in diseases of the lungs, especially when complicated with the extreme debility of fever, when the functions of the lungs are gradually impeded, the colour of the blood is changed in all the tissues of the body; but as death does not ensue, until, as in common asphyxia, the circulation has been arrested from cessation of the functions of the lung, this blood has no sudden noxious influence on the various tissues. Hunter, however, discovered, that the colour of the blood in contact with any living tissue, gradually becomes darker, until it has assumed the venous colour. After death, on what lesion soever it may depend, we never discover bright arterial blood in any tissue of the body. The state of the vascular system differs also in asphyxia from that observed in other kinds of death, as the blood is chiefly accumulated in the veins, and the radiculæ and minute vessels terminating in them. That the tissues therefore should assume a somewhat livid colour, is not so peculiar that it must be ascribed to an universal morbid change in the blood, which cannot happen when the respiration is suddenly interrupted; but it must be attributed to the strange influence of tissue upon blood, by which its qualities assume in a greater degree the venous characters, when surrounded by it, and to the peculiar state of congestion of the venous system, rendering this colour more obvious to the eye.

Bichat proceeds (p. 275,) to attempt to prove "that the dark blood is incapable of supporting the activity and the life of the organs which it penetrates, when the functions of the lung have ceased." The arterial blood, he remarks, is peculiarly adapted as a vital stimulus to the various organs of the body; (p. 275-276,) and (p. 277,) he speculates concerning the method by which it supports the various organic motions. He then proposes (p. 278,) to examine how the contact of the dark blood with the structures occasions their death. The action of all the organs of what Bichat has termed animal life, depends on the brain, and, supporting his opinion by his experiments concerning the influence of the dark blood on the brain, he supposes that it is through this medium fatal to them. "But when," he adds, "this fluid circulates in the general system, when all the organs are subjected to its influence, two other causes are added to this. 1. The nerves which are pene-

trated by it are no longer capable of establishing communications between the brain and the senses on the one hand, and on the other, between this viscus, and the locomotive and vocal organs. 2. The contact of the dark blood with the organs themselves, destroys their action." For he asserts, if the dark blood be injected into the crural artery of an animal, paralysis supervenes. He repeats these opinions in page 280. But its influence on the nerves rests simply on the opinion of Bichat, unsupported by experiment, and we have shown that it has no noxious influence on the muscle. Pursuing the argument, he remarks the difficulty of proving satisfactorily "that the secretions, exhalation, and nutrition, cannot obtain from the dark blood the materials necessary to support them, for this blood does not circulate in the arteries, during a period sufficiently long, to enable us to make experiments on these functions. He then remarks, that, after respiration had been suspended, he had never witnessed the flow of semen from the *vas deferens*, or of urine from the ureter, but he allows that no consequence can be adduced from observations of this nature, made during the short sufferings of the animal. "It is therefore," he continues, "principally from the analogy of that which happens in other organs, that we determine that the functions of secretion, exhalation, and nutrition, cease when the dark blood penetrates their structures." But upon what analogy? The only positive experiments, adduced in support of these opinions, concern the effect of the dark blood on the muscle and the brain. The first we have shown to be erroneous; and concerning the second we can only at present remark, that, allowing the experiment to be correct, the fact that the dark blood supports and revives the contractility of muscle, demonstrates that no analogy can be drawn between the brain and other organs. Analogy cannot therefore be adduced in aid of this opinion.

The argument of Bichat here ceases; and it is needless to follow him through the subsequent pages, in which he attempts to trace a vague accordance between his opinions and some of the obscure and even doubtful phenomena of asphyxia.

To these opinions it must further be objected, that in young animals, as is proved by the experiments of Buffon and Edwards, and in animals of cold blood, life, with all its functions and phenomena, is supported for a considerable period by the presence of the dark blood; and as the hypothesis of Bichat is supported by no valid experiment or argument, we are inclined to the opinion, that, though the dark blood is doubtless less favourable to the action of the organs generally, than the arterial, yet its presence exerts no positively noxious influence upon them, but is even capable of supporting their actions for a certain period. The evidence of more decided experiment is desirable to confirm this opinion, as far as it respects the organic motions generally. Concerning muscle, the experiments detailed in this inquiry are positive; and the

blood being necessary to secretion and nutrition, it is a natural consequence, that, when the circulation is arrested, as in asphyxia, these functions should cease.

I embrace this opportunity of expressing my great obligation to my enterprising colleagues, Drs. Nesbitt and Bruce, for their very valuable assistance in some experiments, and for their watchful scrutiny during the progress of almost all.

From the London Medical and Physical Journal.

VACCINATION IN AFRICA.

On the departure of Lieutenant James Holman, R. N. the celebrated blind traveller, for Africa, I begged the favour of him to procure for me some authentic information respecting the present state of small pox and vaccination in the Negro country, and I supplied him with a few queries calculated to direct his inquiries. He has just transmitted to me the accompanying communication from W. Ferguson, Esq. surgeon of the Royal African Colonial Corps, which appears to me to possess sufficient interest to merit a place in the pages of your excellent Journal.

I am your obedient servant,

GEORGE GREGORY.

London, December 19, 1828.

Replies to Queries on Vaccination and Small Pox. By W. FERGUSON, Surgeon Roy. Af. C. C.

1st. Is vaccination generally practised among the infant negro population?

2d. Whence do they derive their stock of lymph?

3d. What is the degree of confidence placed in it?

Vaccination is not at all practised among the negro population by native vaccinators; it is, however, practised among certain branches of the negro population by European surgeons. The negro population of Sierra Leone consists of Nova Scotian and Macroon settlers, liberated Africans, and several of the aboriginal African tribes, such as Limmaners, Mandingoes, Soosoos, Sherbros, &c. &c. &c. The three first mentioned of these branches of the negro population, having greater intercourse with Europeans, are better acquainted with European customs, and have, of course, imbibed more of European notions and prejudices on such subjects as the one now under consideration, than the aboriginal inhabitants of this part of Africa: vaccination therefore is, and has been, practised among them to a considerable extent; the stock of lymph being derived from, and kept up by frequent renewal from England. That their confidence in it as a measure preventive of small pox is great, I judge from the anxiety which they show, and the eagerness which they manifest to have their children vaccinated, when small pox is raging round them; while, under ordinary circumstances, and when their fears have

been lulled by the absence of this fatal epidemic, (an absence which they well know is probably but temporary,) they exhibit such an unaccountable apathy regarding vaccination, that a stranger might well suppose they had no faith in it at all as a prophylactic measure. Notwithstanding this, I believe they have great confidence in it, although, from circumstances to which I shall presently allude, that confidence has, I believe, declined considerably.

4th. How soon does the areola arrive at its greatest height in these countries?

The areola surrounding the vaccine vesicle is, I think, at its greatest height about the eleventh or twelfth day after vaccination, if the lymph used has been genuine.

5th. Does small pox prevail there?

6th. Does small pox prevail there after vaccination?

Small pox prevails occasionally, and there are instances of its having occurred even in a confluent form after vaccination. One genuine instance of this kind came under my notice in the year 1824, in the person of a liberated African girl, of about sixteen years of age. Vaccination had been performed in this case by the late Dr. Nicol, deputy inspector of hospitals, and was considered satisfactory. The case proved confluent. The secondary fever was accompanied by severe diarrhoea, which carried off the patient about the thirteenth day. Another well authenticated instance of the same fact occurred in the early part of the present year, in the family of a respectable Nova Scotian settler. Other cases of a similar nature have been reported by the inhabitants; but I do not consider that in these cases the proofs of a pure previous vaccine disease have been satisfactorily established. When vaccination has been carried on for some time from the same stock of lymph, the disease is apt to degenerate and become spurious; from which cause we require a frequent renewal of lymph from England, in order to keep it in continuous and successful operation. The spurious disease, on the fifth day, generally shows itself in the form of a small globated papula; on the eighth day, it presents sometimes an ash-coloured pustule, containing purulent matter; at other times, and less frequently, a brown-coloured scab, having a small quantity of purulent matter under it, capable of producing, by inoculation, a disease similar to itself. The great prevalence of a disease among the negro population called *Craw Craw*, is considered as materially influencing that change in the properties of the pure vaccine lymph which has been just noticed. That apathy and indolence, of which I have already accused the negro population, leads them to consider the appearance of disease in the arm after vaccination, as the test of safety from small pox. Great as the difficulty sometimes is in getting them to bring forward their children for vaccination, it is still greater to procure the necessary examinations in its progress and maturation. The mere appearance of disease in the arm is sup-

posed to carry along with it immunity from small pox; and, on the occurrence of the epidemic at an after period, it may be easily foreseen how wretchedly and how fatally this confidence in the spurious disease may be misplaced. I, therefore, do not consider that, in all the cases spoken of among the inhabitants as cases of small pox occurring after vaccination, there existed satisfactory proofs of the patient's having previously undergone the genuine vaccine disease; yet I am sorry to say that, from such occurrences as these, vaccination has rather lost ground in the opinion of the negro population.

7th. Is small pox an increasing malady?

Small pox is not an increasing malady: it is generally introduced here from the slave cargoes of vessels detained by the squadron, and sent here for adjudication. Were this source of its renewal removed, I am persuaded that small pox would, in the course of a few years, be almost unknown in this part of Africa.

8th. Can the vaccine virus be retained on points and glasses, so as to be fit for use?

The vaccine lymph, if taken on points, will not retain its power seven days in this country. This observation is established by repeated trials. If taken on glasses, I would not be disposed to depend on its activity when kept longer than fourteen or sixteen days, though I have known it sometimes to retain its original properties for four or five weeks. If preserved in glass bulbs hermetically sealed in the manner practised by the National Vaccine Institution, I have known its properties unimpaired after keeping for three months. Repeated trials have convinced me of the excellence of this mode of preserving the vaccine lymph, and I believe it to be the best and surest that has been yet devised, of transmitting the lymph from England to tropical countries: next to this method, I believe the crusts have proved most successful.

9th. Are there periodical vaccinations of large districts, or is each child vaccinated soon after its birth: if the latter, how soon?

The practice in these cases is, as long as the vaccine lymph continues to produce a genuine disease, to keep it up by the weekly vaccination of all comers. Children are rarely vaccinated under four weeks old, but there is no rule observed on this head.

10th. What sort of scars are usually left in the arms?

The scar bears the shape of the original vesicle, and is slightly depressed below the surface of the surrounding skin. The surface of the scar is marked by a number of small depressions of various shapes, corresponding, I believe, with the cells in the original vesicle.

11th. Is vaccination in hot countries attended with feverish symptoms? If it is, on what day do they begin?

Vaccination is sometimes in this country attended with feverish symptoms; but, in the most marked cases, so far as I have seen, these symptoms have been so slight as almost to escape common observation. I have not remarked on what day they begin.

12th. Is vaccination ever followed by any eruptions?

I have seen only one case of this: an eruption appeared on the sixth day after unsuccessful vaccination: it was diffused over the whole body, and is now in progress.

Sierra Leone, September 24, 1827.

N. B.—The case alluded to in the last of the above replies, was in the first instance a papular eruption, the base of each papula being surrounded by an inflamed ring. The eruption was thickest on the thorax and on the arms. In its progress the eruption became pustular, the pustules being in circumference about half the usual size of the vaccine vesicle. On the twelfth day, the crusts had dropped from some of the smaller pustules; and, by the seventeenth day, they had all dropped off, leaving a mark, but not in any manner pitted, and which I do not think promises to be permanent.

From the London Medical and Physical Journal.

OBSERVATIONS ON THE NATURE AND TREATMENT OF BERIBERI. By WILLIAM HAMILTON, Esq. Royal Navy.

By the different authors who have touched upon the subject, from the days of Bontius (who first described the disease, and gave it a place in medical nomenclature,) down to those of Dr. Christie, and from his time to the present, Beriberi has been considered as a disease of debility, in the cure of which any thing approaching to a depletory mode of treatment, has been most religiously avoided. Finding, from a medical friend lately returned from India, that little or no deviation from the accustomed practice so long pursued in such cases had taken place at the time he quitted that country, in June, 1825,—a short time previously to which beriberi had proved more than usually fatal, particularly in the northern division of the Madras settlement,—I have, at the request of the individual alluded to, been induced to communicate, through your Journal, the following observations on the nature and treatment of this complaint, as tending to favour the propriety of a very opposite mode of practice to that so generally had recourse to for its cure. My object being chiefly to confine myself to facts, and by a simple statement of such lead the attention of others to the subject, I shall, in the course of my remarks, avoid, in as far as possible, touching upon such points as would naturally involve either hypothesis or theory.

This very singular endemic (occurring in particular parts of the East Indies, and which, I believe, has not been described as having existed in any other quarter of the world,) appears to be principally, I may say almost entirely, confined to the Island of Ceylon, the Malabar coast, and that tract of country reaching from Madras as far north as Ganjam, and in no part extending inland more than forty

miles, forming what is called the northern division of the Madras settlement. It is most prevalent during the decline of one monsoon and setting-in of the other, when the atmosphere is completely loaded with cold, raw, damp vapours, and the vicissitudes of temperature are greater than at any other period of the year; and though in a less degree does a residence in the neighbourhood of the coast seem essential for its production, than a stay for some months on a station where it prevails, yet the instances are comparatively rare where it has been found to occur at a distance from the sea exceeding sixty or seventy miles.

Individuals of very different constitutions and habits of body seem liable to be affected by it; but such as lead a sedentary and debauched life, and who are much exposed to vicissitudes of weather and sudden changes of temperature, are unquestionably those most subject to its attacks; and an individual once having suffered from it, appears more liable to a future attack than one who had never been the subject of it.

I have only had an opportunity of seeing the disease under two forms, viz. that in which the symptoms were at first mild, and gradually increased in severity; and that in which the symptoms were even from the first rather urgent, increased more rapidly, and, unless speedily relieved, soon led to the most alarming appearances, and finally proved fatal.—There is another variety mentioned by Dr. Christie, which that author considers equally fatal, and in which “there was not any swelling observable externally, but the patient, with the other symptoms, had evidently the bloated leucophlegmatic face of a dropsical person.” Dr. Rogers, of the Hon. East India Company’s Madras establishment, likewise mentions this variety of the complaint, in a Thesis written by him, and printed at Edinburgh in the year 1808. “Hydrops asthmaticus,” says he, “tumore nullo externo comitante, nobis ante oculos interdum versatus fuit; his autem in exemplis ægro exitum semper attulit; vultusque et tumidus et leucophlegmaticus, hydropæ laborantis sane proprius ægro tanti fuit.” This form of the complaint I have never seen. The first case that fell under my care was one in which the disease appeared under its milder form. There was from the commencement slight dyspnoea, with stiffness of the lower extremities; pulse quick and sharp; skin dry and rough. Supposing it only a slight feverish attack, (the individual having been exposed the preceding night to the damp and cold winds which prevail during the change of monsoon,) I prescribed a solution of emetic tartar, to be given in small repeated doses, for the purpose of acting on the stomach, bowels, and skin, and directed that he should be kept warm; having very little doubt that, by next morning, I should find my patient much relieved. To my great mortification, however, I found all the symptoms much aggravated, particularly the stiffness he complained of in his legs and thighs, which now

amounted to an almost complete paralysis. From this time there was no mistaking the nature of the complaint, and, though I strictly followed the plan of treatment recommended by Dr. Christie, and by him found so successful, the poor fellow died on the evening of the tenth day from the commencement of the attack, having for some days previously laboured under all the symptoms generally present in beriberi. This body, from particular circumstances, I was prevented examining.

In the second case which I had an opportunity of attending, the disease, from the time that I first saw it, appeared under its more aggravated form. The symptoms were as follows:—Great debility, with difficulty of respiration; a sense of weight and oppression at the lower end of the sternum; and an almost paralytic state of the thighs and legs, which, soon after the commencement of the attack, became œdematous, as did also the face, and indeed the greater part of the body; with a general sensation of coldness over its surface; pulse 120, small, feeble, and intermitting. All these symptoms went on increasing until the death of the patient, which took place within forty-eight hours from the time he was first attacked. A short time previously to his death, he was seized with a violent fit of vomiting, spasms of the abdominal muscles, and increased dyspnœa, which carried him off.

Though the plan of treatment recommended by Dr. Christie had in the first case proved unsuccessful, such was my confidence in that author, from the very decided manner in which he speaks of his success, that I was a second time induced to follow his footsteps in the treatment of this complaint; but, I am sorry to say, as unsuccessfully as before. I now determined to examine the body, and, if possible, discover some indication or other by which I might be guided in my future treatment of the complaint, should a case of the kind again occur to me. I shall here state the particulars of this examination, together with the mode of treatment I was afterwards led to adopt, by which I succeeded in completely curing my next three patients; in two of whom the symptoms were, in the first instance, still more alarming than in either of the cases which terminated fatally. Leaving the country soon afterwards, I was deprived of an opportunity of giving it a more extensive trial.

I carefully examined the contents of the cranium, thorax, and abdomen. Upon removing the scull-cap, I found upwards of an ounce of serum effused between the dura mater and tunica arachnoidea; and in two or three different places there appeared dark red-coloured patches, one of which was exceedingly vascular, and extended into the substance of the brain, to the depth of from a quarter to half an inch. There was likewise found considerable effusion in all of the ventricles except the fifth, or that cavity formed by the separation of the laminae of the septum lucidum. In the base of the cranium, upon the brain being removed, there appeared upwards of four ounces of fluid tinged with blood. The lungs

were very much loaded with dark-coloured blood; and in both cavities of the thorax there was found extensive effusion. The heart was of a pretty healthy appearance; nor did the pericardium seem to contain a much greater quantity of fluid than that generally found in it; both on its external surface, however, and likewise internally, there existed very evident marks of inflammation. The diaphragm, particularly towards the right side, appeared considerably inflamed. The stomach was of a healthy appearance, and contained about six ounces of a dark brown-coloured liquid. The liver was very evidently larger than natural, and appeared even still more loaded than the lungs: on cutting through its substance, the blood, from different points, trickled out almost in a continued stream; and indeed all, even the most minute vessels, seemed completely gorged; as were likewise those of the mesentery and pancreas. In several places on the surface of the intestines there appeared a sort of efflorescence, but, upon the whole, they presented nothing very remarkable. From three to four pounds of fluid were found in the cavity of the abdomen; and in the cellular texture, almost all over the body, there existed very extensive effusion. On examining the spinal marrow, there was found to exist the same evident marks of congestion as in the liver, lungs, &c. almost along the whole of its course, though that more particularly in the dorsal region. No other deviation from the natural healthy appearance of the parts was found to exist.

Dr. Christie mentions, that in many of the subjects he examined who had fallen victims to this disease, there was a remarkable obesity, even after a long continuance of it, and the use of mercury, antimony, and other powerful medicines. This I found to be the case in the individual whose body I examined; but he was naturally of a full habit of body, and in him the disease had not existed long.

Having fully satisfied myself, from the post-mortem appearances, that the dropsical affection and other symptoms in the case of the individual whose body I had an opportunity of examining, (and I have little doubt in every other where the disease occurs,) arose from obstructed circulation, in consequence of congestion in the internal organs; and that beriberi consequently could not be considered a mere disease of debility, as described by the celebrated Bontius, and more recently by Dr. Christie, Mr. Hunter, Mr. Collinson, and others, but a disease in the treatment of which depletion might be had recourse to with the greatest possible advantage, I fully determined, should an opportunity again present itself, (regardless of the apparent debility of the patient on the one hand, or of the doctrines of the humoral pathology on the other,) to try the effects of blood-letting, for the purpose of assisting other means in exciting reaction, determining towards the surface, and restoring the balance of circulation. For this end, in the following case, thirty ounces of blood were, upon my first seeing the patient, taken from

the arm *pleno rivo*; which, from the immediate relief it afforded, I was induced to repeat: and accordingly, about twelve hours from the first bleeding, the dyspnœa and other symptoms, though much relieved, still continuing troublesome, twenty-five ounces more were drawn off in a full stream, which was followed by the same evident good effects as on the former occasion. I felt inclined even a third time to repeat the bleeding, and certainly would, had the individual been of a full habit of body, or had the disease occurred to me in a climate where debility and general relaxation were less liable to follow free depletion, and where a practitioner might, with comparative safety, carry the use of a lancet to a greater height than he would be warranted doing in a climate such as that of India.

Having resolved not to push venesection further, I had immediate recourse to mercury, upon which I now determined principally to rely in the cure of the disease, having every hope of saving my patient, provided I could succeed in bringing him speedily and effectually under its influence. Its well known effects as a great, universal, and permanent vascular stimulus, as an equaliser of the circulation, and as taking off determination from particular parts,—its diuretic effects when combined with diuretics, and diaphoretic when combined with diaphoretics, render it a medicine peculiarly well adapted for the cure of beriberi.

Soon after the second bleeding, I directed twenty grains of calomel, with thirty drops of laudanum, to be given, and had the patient laid upon the frame of a common sea-cot, having an open ratan bottom, under each end of which was placed a block of wood, for the purpose of raising it a certain distance from the ground. A blanket, having a hole cut in it sufficiently large for allowing the head to protrude, was now thrown over all, and brought close down to the ground at the ends on each side. Two crucibles, containing ignited charcoal, were then placed under it; by means of which the whole surface of the body was freely exposed to the fumes of the hydrargyri oxydum cinerium, some of which was from time to time thrown into the crucibles. This I continued for upwards of half an hour, when, the patient feeling faint, the crucibles were removed, and the body enveloped with the blanket which covered it during the fumigation.

An hour and ten minutes having elapsed from the time the calomel was given, the dose was now repeated, with the same quantity of laudanum as before: soon after which he fell into a sound sleep, and continued so for upwards of three hours. Upon his waking, I found him in a state of copious perspiration, the pulse increased in strength, and the dyspnœa not near so troublesome. I now again repeated the calomel, with six grains of gamboge, for the purpose of acting on his bowels, and omitted the laudanum; had also the crucibles replaced, and the body again exposed to the fumes of the hydrargyri oxydum ci-

nerium; which, from this time, together with scruple doses of calomel, and friction over the surface of the abdomen and thighs with the unguentum hydrargyri fortius and liquor ammoniæ, was repeated from every three to four hours until ptyalism was fully established, (which, notwithstanding the very active means had recourse to, required from forty to forty-eight hours to effect;) after which every unfavourable symptom began speedily to disappear, and the patient's principal complaint was the soreness of his mouth.

The particulars of this case, which are nearly similar to that of two others which fell under my care, and in which the same mode of treatment proved equally successful, I beg leave to subjoin, as copied from my notes taken at the time.

I may here add that, in one of the cases, there was from the first violent vomiting, which soon, however, yielded to large and repeated doses of calomel and laudanum, together with the application of strong mustard sinapisms, at from 150° to 160° of Fahrenheit, to the region of the stomach; which I have likewise found singularly successful in speedily allaying the violent gastric irritability, in cases of bilious remittent fever and cholera, where the calomel and laudanum alone had completely failed of success. The individual being of a rather full habit of body, and the pulse, after the first and second bleedings, continuing strong, blood-letting was in this case a third time had recourse to; the patient losing in all upwards of sixty-five ounces of blood within the space of thirty hours, and that with the happiest effects, notwithstanding that the dropsical and other symptoms had existed for nearly two days previously to my seeing him, and that the anasarca swelling, in the interval between the bleedings, had very considerably increased.

The above observations were chiefly written during the progress of a voyage from the East Indies to England, in the year 1822; since which time I have had an opportunity of perusing Mr. Marshall's Notes on the Medical Topography of the Interior of Ceylon, in the which mention is made of two or three cases of this disease, where the dyspnœa was very troublesome, and for the relief of which blood letting was had recourse to with immediate advantage. Mr. M. however adds, that "a more extended clinical experience is still necessary before a due estimate can be made of the effects of the depletory means of cure." I have much pleasure in being able to contribute my mite to the testimony of Mr. Marshall in favour of a mode of treatment, which I have no hesitation in saying, whoever would successfully combat this "hydra disease," would do well to adopt.

In addition to the cases mentioned by Mr. Marshall, I am happy at having it in my power to notice two others, with the history of which I have been recently favoured by my friend, Dr. C. Rogers, of Edinburgh, in the treatment of which blood-letting was had recourse to, as in Mr. Marshall's cases, for the purpose of

relieving dyspnœa, and was speedily followed by a striking alleviation of all the symptoms. These cases occurred so late as the year 1823.

This evidence in favour of the depletory mode of cure I consider strong, and the more particularly so coming from such authority as that of Dr. Rogers, who for some time practised along with Dr. Christie in the Island of Ceylon, and lately filled the situation of superintending surgeon in the northern division of the Madras settlement; a station where he had ample opportunities of seeing and treating the disease under its every form, and who not long since entertained very different views on the subject to those which he has lately been led to adopt. In allusion to blood-letting in the treatment of this disease, (as found in his *Disputatio Inauguralis Medica quædam de Hydrope Asthmatico in Ceylonia, grassante Beriberia, dicto Complectence*;) he makes use of the following words:—"Quin et, ad phlebotomiam tandem decursum fuit; vena autem pertusa, morbus cursum ejus funestum celerius absolvit."

CASE.—November 20th, 1820, 10 A. M.—H. C., æt. thirty-two, of a rather spare habit of body, says that he has felt more or less indisposed for the last two days. Complained last night of very acute headach, with a sense of extreme debility and severe dyspnœa, (which last came on him suddenly,) of great numbness, and a disagreeable pricking pain in his thighs and legs, which were much swelled and œdematous; face and abdomen likewise considerably swelled; countenance extremely anxious; complained of feeling very cold; inclined much to vomit; pulse 120, weak and irregular; tongue furred; urine scanty and high coloured; bowels open; skin dry, and cooler than natural. Has lived for the greater part of the last twelve months on the Island of Ceylon. Was bled to the extent of thirty ounces, which afforded much, and almost immediate relief; had also four grains of calomel, with ten grains of jalap, given him, for the purpose of acting upon his bowels. This was, however, soon afterwards vomited; when thirty drops of tinct. opii were given, with a view to moderate the gastric irritability which existed, and procure sleep.

I visited him this morning at ten A. M.—Says that he has passed a very restless night. All the symptoms, though not so urgent as before the bleeding, still continue, particularly the headach and dyspnœa; pulse 116, stronger and rather more regular than at last visit. Blood taken last night of a buffy appearance. Mittantur mox pleno rivo sanguinis uncia quinque viginti.—R. Subm. hydrargyri ʒj.; tinct. opii gtt. iij.; pulv. scillæ gr. j. M. statim sum.

Six P. M.—Feels better: dyspnœa and headach much relieved by the bleeding. The calomel and laudanum twice repeated since morning, with the addition of six grains of gamboge to the last dose, for the purpose of acting on his bowels. Has had the fumes of the hyd. oxyd. ciner. twice applied to the surface of the body, which was each time followed by a general glow of heat and free

perspiration; and, soon after the first application, completely removed that coldness of the extremities, and indeed of the whole body, from which he appeared to suffer much at the time I first saw him. Œdematous swelling now almost general over the whole surface of the body; pulse ninety-six, softer, and more regular; thirst urgent. Blood taken in the morning slightly buffy. Omittantur tinctura opii.—Repet. hydrag. submur. et pulv. scillæ tertia quâque horâ.—Suffit. quartâ quâque horâ.—R. unguent. hyd. ʒij.; aq. ammon. ʒj. M. infric. semi uncia de die.—R. supertart. potassæ ʒj. infunde in aquæ ferventis unciis quindecim per semihoram et cola. Capiat uncias tres pro re nata.

21st.—Has had several copious watery stools since yesterday evening; slept badly; dyspnœa less troublesome; headach better; œdema of extremities slightly diminished; urine still scanty and very high coloured; great apparent debility; thirst incessant; pulse ninety-eight, and pretty full; perspires freely for some time after each fumigation; countenance bloated; tongue furred. Has taken twenty grains of calomel almost every three hours since yesterday morning; had the fumes of the hydrargyri oxydum cinerium regularly applied to the surface of the body, and the ung. hyd. fort. repeatedly rubbed in during the night, but as yet there is no appearance of ptyalism. Continuentur medicamenta ut heri præscripta.

Six P. M.—Dyspnœa very much relieved; anasarca swelling of the abdomen considerably reduced, but the œdema of the lower extremities appears very little diminished; thirst still urgent. Drinks freely of the cream of tartar and water, which keeps his bowels open. Has made upwards of two pounds of urine during the day, not so high coloured as formerly; breath affected by the mercury. Diet, light soup and sago. Continuentur omnia.

22d.—Spits copiously; slept little during the night, but there is to-day a most striking alteration for the better: dyspnœa almost entirely gone; anasarca swelling all over the body very perceptibly diminished; thirst less urgent. Has made nearly six pounds of urine since yesterday afternoon. At times perspires freely; pulse 102; tongue rather loaded; bowels loose. Says he feels much stronger than he has before done since the commencement of the attack; appetite pretty good. Diet, soup and sago. Omittantur omnia medicamenta.—Utatur gargarism ex aq. font. et nitrat. potassæ.

Six. P. M.—Complains chiefly of the soreness of his mouth; little or no difficulty of respiration. Quantity of urine since morning, eight pounds. Skin moist; pulse ninety, and pretty strong; no stool during the day. Had a little boiled fowl for dinner. R. Potassæ supertart. ʒ vj.; pulv. jalap. gr. x. M. fiat pulv. statim sumend.

23d.—Until about four A. M. rested well; since which time he has had frequent watery stools, from the cream of tartar and jalap. Saliva flowing freely; no return of dyspnœa;

œdema rapidly diminishing; appetite getting keen; still complains of thirst; pulse eighty-six; urine copious; skin dry. Diet; fowl, soup, and sago. Cont. garg.—Habeat decoctum hordei pro potu ordinario.—R. Infus. gent. comp. ℥ij.; liquor ammon. acetat. ℥ss.; spirit. ætheris nit. ℥ij. M. fiat haust. ter in die sumend.—Habeat ter in die spirit vin. Holland. unciam ex aq.

Six P. M.—Continues doing well; has had three copious liquid stools since morning. Skin dry and rough; urine, during the last ten hours, upwards of three pounds; pulse ninety-two. Continuentur medicamenta ut heri, et capiat hora somni.—Opii gr. iv.; pulv. ipecac. gr. iv.; sub. sulph. hyd. flav. gr. ij. M. fiat pil. ij.*

24th.—Slept well, and has perspired very copiously during the night, so much so, that it became necessary this morning to remove the blankets which were next him. Œdema daily decreasing; countenance becoming much more natural; mouth continues sore; appetite good; pulse eighty-two. Continuentur garg., decoct. hord., mist. diuret., et sp. vin. Holland.

Six P. M.—Has had no stool for the last twenty-four hours; skin moist; pulse ninety. R. Pulv. jalap. gr. x.; supurtart. potassæ ℥iv. M.

25th.—Appears to-day much better in every respect; has had three copious liquid stools from the purgative. Œdema of the extremities entirely reduced, that of the abdomen so much so that it does not now pit on pressure. Still spits freely; urine of a more natural appearance, and begins to diminish in quantity; appetite very keen; thirst much less urgent; skin moist; pulse eighty-two. Diet as before. Continuentur medicamenta.

26th.—Has had a good night. Anasarcous swelling all over the body completely gone. Says that he feels daily getting stronger. but has still considerable numbness of the lower extremities, particularly felt upon attempting to walk; mouth getting better. Has flannel bandages tightly applied from the toes up to the groin on each side, and the parts well rubbed with stimulating liniments. Habeat bis in die vin. alb. ℥ij.—Continuentur alia ut heri.

27th.—Continues improving: says his appetite is now so keen that he can scarcely satiate it; general appearance good; skin moist; pulse eighty-six and strong; bowels open. Had a blister applied last night along the course of the spine, in the dorsal region, which has risen well. Continuentur medicamenta.

28th.—Complains only of the soreness of his mouth, which is, however, getting fast well. Walked for nearly a quarter of an hour yesterday, without feeling much fatigued. Continuentur medicamenta.

29th.—Improves in strength daily. No stool for the last twenty-four hours; skin moist;

pulse natural. R. Pulv. jalap. gr. vj.; supurtart. potassæ ℥ij. M. statim sumend.—Contr. alia ut heri.

30th.—Makes little or no complaint to-day: mouth almost well. Has had two stools from the cream of tartar and jalap given yesterday. Contr. medicamenta.

December 1st.—During the last two days has wonderfully improved in appearance. Appetite continues good: pulse regular; skin moist; bowels open; tongue clean; voids about the natural quantity of urine in the twenty-four hours. Contr. medicamenta.

2d.—Perfectly well, except a slight numbness of the inferior extremities, which still continues. Contr. medicamenta.

3d.—Had a second blister applied last night, about two inches broad, along the whole course of the spine, which has risen well. Curetur ulcus ungt. sabin.—Contr. medicamenta.

4th.—Continues well in his general health. Habeat vini libram indies.—Omittantur alia.

5th.—Discharged from the sick report, with instructions not to resume his duty until the numbness of the extremities shall have been completely removed.

This individual, in about ten days from the time that he was discharged, had perfectly recovered the use of his limbs, and was in every respect completely cured.

From the London Medical Gazette.

ON THE HIGH OPERATION OF LITHOTOMY. BY M. DUPUYTREN.

M. Dupuytren thinks the above operation ought not to be employed as a general method; that is to say, in all cases indiscriminately, without regard to age, sex, or constitution, or without reference to the probable size of the calculus; but that it should only be had recourse to either when the tuberosities of the ischium approach each other more than usual, when tumours are situated in the lower strait of the pelvis, or where the great size of the stone would render its extraction difficult, or impossible, from below. M. Dupuytren supports his opinion by quoting the result of Frère Come's experience by this method of operating, in eighty-four individuals of different sexes, of all ages, constitutions, and states of health; by which it appears that one out of $4\frac{1}{2}$ of the whole number, perished. This appears much more unfavourable than the result obtained by the subpubic operation practised indifferently, as in the experiment of Frère Come, upon children, women, adults, and old men. As to the instruments and mode of operating employed in these cases by Frère Come, M. Dupuytren, whilst he admits their real advantages, is far from considering them as obviating all the dangers attending the operation. In a thesis sustained by M. Dupuytren, in the year 1812, occasioned by the competition for the chair of operative medicine, vacant by the death of Sabatier, he gave it as his opinion that the incisions made in the perinæum, and in the neck of the bladder, added to the dangers of the high operation all those of the lateral one. More recently he expressed

* The above description of the late Dr. Marryat is perhaps the most effectual diaphoretic that can be given, and at the same time mild in its operation.

this opinion, that a sound placed in the bladder, either by the perinæum or by the urethra, added to the inconvenience and danger of the high operation, without being productive of any marked advantages in the re-union of the parts. These two opinions have, however, been for some time the object of controversy among practitioners. Some maintained that an incision made in the perinæum, if of small dimensions, added nothing whatever to the danger of the hypogastric operation, and that it afforded considerable facility to the introduction and management of the *sonde à dard*, which they thought indispensable in the performance of the high operation. They added, that this incision, terminating at the most depending part of the bladder, presented a ready exit for the urine; thus diverting it, as it were, from the upper opening made in the abdomen.

Among the objections made to M. Dupuytren's opinion, that which consisted in saying that the incision in the perinæum and neck of the bladder added nothing to the dangers of the operation, fell before the results of the lateral method, which, according to age, season, or other circumstances, is mortal in the proportion of one individual to five or six. Now, what is the opening made in the perinæum, in the high operation, but adding the lateral operation also? It is in vain to say that this incision, made as small as possible, is much more free from danger than the lateral method, for we know too well that the danger of this incision bears no proportion to its extent. The objection drawn from the facility which this incision gives in introducing and managing the *sonde à dard*, is a little more specious; in fact, it cannot be denied (if the *sonde à dard* be really indispensable) but that it may be more easily made use of by introducing it into the bladder by means of an incision in the perinæum, but first it may be used by introducing it by the urethra, as M. Dupuytren has proved upon the living subject many times. In order to do this, it has been sufficient to give its extremity a greater degree of curvature than ordinary, and to lower the handle more forcibly after having introduced it into the bladder, which, owing to the flexibility of the urethra, is easily done. The *sonde à dard* is any thing but indispensable to the performance of the high operation, as we shall prove from among a number of similar cases.

As to the facility which the opening into the perinæum affords to the flow of the urine, it is easy to conceive that an instrument introduced into the bladder, through the urethra, fulfils this indication equally well, and with less danger, than the incision in the perinæum. But the belief that an incision so made, and an instrument thus introduced into the bladder, prevent the escape of the urine by the upper wound, is a supposition which, according to M. Dupuytren, experience and observation daily contradict. In fact, whatever may be the size of the incision made in the perinæum—whatever may be the caliber of the catheter passed by the urethra—we never find the urine completely withdrawn from the opening made

in the bladder above the pubes; it flows always from both openings. It would appear, to use M. Dupuytren's expression, that whenever the bladder is wounded in any part, that point becomes the centre towards which all the contractions of this organ are directed, and consequently the urine is expelled by these contractions; so that he has long looked upon all the precautions taken to prevent the urine from escaping by the wound made into the bladder, as useless. He goes further, for he regards all such attempts as dangerous; such as apposition, suture, compression, and other similar means;—he thinks that they are only calculated to produce infiltrations of urine, and consequent inflammation either of the abdomen or of the cellular tissue of the pelvis, two of the most unfortunate accidents to which lithotomy gives rise. He, therefore, believes it to be the best plan, after having performed the operation above the pubes, to leave the wound to itself, merely keeping the edges apart, and observing a relaxed position of the body. The relation of the following case will explain M. Dupuytren's mode of operating better than all the general remarks that could be offered.

M. Leroy, 62 years of age, an inhabitant of Chantilly, and architect to the late Prince de Condé; of a middle height, strong constitution, and sanguine temperament; accustomed to great fatigue, and very active; who had always made use of a rich diet and generous wines, had suffered for a considerable time from a frequent desire to make water, difficulty in passing it, and severe pains after having emptied the bladder; to these symptoms attacks of hæmaturia had been added, whenever he had taken more exercise than ordinary, or committed an excess in eating or drinking. During the two last years, all these symptoms had become aggravated, the flow of urine was almost perpetual and involuntary, and there was continual pain both in the hypogastrium and perinæum. At this period a surgeon sounded the patient, but discovered no calculus. At the end of a year another surgeon pronounced the disease to be a catarrh of the bladder, and prescribed a plan of treatment accordingly: nevertheless, the condition of the patient grew worse from day to day; the water came away only drop by drop, after violent efforts, and accompanied by great pain. Worn out with suffering, the patient was obliged, in spite of his resolution, to keep his bed, where he was soon attacked with a slow fever, and fell away perceptibly. The urine at this time gave out a strong ammoniacal odour, and seemed composed of a mixture of urine, blood, pus, and mucus. It was in 1824 M. Dupuytren was called to see the patient at Chantilly. Upon the relation of the above symptoms, he foretold the existence of a stone in the bladder, and that it was either very large, or entangled in the neck of the viscus. He sounded the patient, and the beak of the instrument struck against a stone before it entered the cavity of the bladder. M. Dupuytren made several attempts to pass the instru-

ment further on, and he only succeeded with difficulty and in part; the sound placed between the stone and the parietes of the bladder, appeared to be squeezed as in a vice. He withdrew the sound, placed his finger in the rectum, and found the lower portion of the bladder dilated, and rendered hard by the presence of the foreign body. After bending the trunk of the body, he next examined the hypogastrium, and discovered below the pubes, at the base of the median abdominal line, a hard, resisting, and voluminous body, which he could alternately raise and depress, by placing one finger on the rectum, and the other hand on the hypogastrium. The urine which was shown to him appeared to be purulent; but the inflammation of which it was the product, was, in his opinion, only an effect of the presence of the calculus, which would disappear when that was removed. The size of the stone determined M. Dupuytren to perform the high operation. A bath and a mild aperient were administered, and on the following night the operation was performed by M. D., assisted by MM. Sansons, Lemaire, Marx, and Dr. Souze. The pubes being shaved, and the patient laid upon his bed, the bladder, hypogastrium, and rectum, were again examined: this examination having confirmed the previous one, M. Dupuytren, placing himself on the right side of the patient, introduced a common silver catheter through the urethra; but this being stopped by the stone, at the neck of the bladder, could penetrate no farther. Water was injected through the catheter, in the hope that it would open the passage, but it passed out again as fast as it was introduced. A *sonde a dard* was substituted for the catheter, but this succeeded no better; and, therefore, M. Dupuytren at once made up his mind to dispense both with the injection and the *sonde a dard*, as he had for a long time done with respect to the incision in the perinaeum: changing his position, therefore, to the left side, and placing M. Sanson on the right, he caused the patient to bend his legs upon his thighs, and then upon the trunk of the body. M. Sanson then introduced his fore finger into the rectum; raised up the stone, and forced it to project as much as possible above the pubes. M. Dupuytren then taking a bistoury, whose cutting edge was convex, commenced an incision level with the upper edge of the symphysis pubes; which he continued in a direction towards the umbilicus, in the median line, for the space of three inches: the skin and cellular membrane, tolerably loaded with fat, being cut through, and the aponeurosis of the muscles being laid bare, and likewise divided, the pyramidal muscles were separated, and the recti muscles, which were by the force of their contractions pressed against each other, divided across for the space of a few lines on each side: the operator's finger passed into the wound, felt the stone, and it was still more sensible to the touch each time that M. Sanson pressed it upwards from the rectum. Resting the cubital edge of the fore finger of his left hand on the symphysis of the pubes, and

sliding the point of a straight and sharp-pointed bistoury along the nail of the above finger, he plunged this instrument into the anterior part of the bladder, immediately behind the symphysis pubes: instantly, a discharge of white, thick, inodorous pus rose from the bottom of the wound. M. Dupuytren thought that this discharge proceeded either from an abscess in the loose cellular tissue about the bladder, or in the thickened substance of that organ; however, the point of the instrument had touched the stone, and therefore entered the bladder: this opening was enlarged from below upwards, to the extent of five or six lines: the fore finger, which had been employed to direct the cutting instrument, was pushed on in the same direction, and entered the bladder with it. The bladder appeared to be half an inch in thickness, and its cavity was filled with an enormous calculus. It was with great satisfaction the operator observed that an interval of two inches existed between the highest point of his incision and the upper part of the bladder not covered with peritoneum; he therefore prolonged the incision by means of a button-pointed bistoury. He then proceeded to the extraction of the stone, which appeared fixed by its mass and shape, and by the contractions of the bladder. M. Dupuytren took a pair of forceps, the handles of which he took apart from each other, and placed them one after the other upon the opposite side of the calculus; and after reuniting them, he, by moving it from left to right, and upwards and downwards, loosened it; and then, causing M. Sanson to raise it up forcibly, he drew it forth. Its form was exactly that of the bladder, which it entirely filled; its dimensions were *three inches and a half in length, three inches in breadth, and two and a half in thickness*. it weighed six ounces and a half, and was composed of ammoniocal magnesian phosphate. M. Dupuytren then examined the bladder, and threw in an emollient injection, which was readily discharged by the wound. The dressing was of the simplest description: the patient felt immediately relieved; but in the course of the night pain in the abdomen came on, attended with cold chills, succeeded by heat and fever, together with nausea and hiccup. A large bleeding from the arm, which was repeated the next day, removed these symptoms. On the third day some urine escaped by the urethra; the next day suppuration of the wound was established, and in about a month the cure was complete; since which time the patient has remained quite well. M. Dupuytren observed in conclusion, that in other cases it would be desirable to distend the bladder by means of an injection of warm water.

From the London Medical and Physical Journal.

OBSERVATIONS ON FEVER. Addressed to a Medical Friend, by Dr. Bow, of Alnwick.

According to promise, my subject must be the theory of fever, although it is one I would

willingly decline; for I now think I was presumptuous when I proposed to enter a field where so many able men have toiled. Nevertheless, the sources of those symptoms to which the term fever is attached, are still concealed; and, since profound research, skill, and ingenuity have failed, chance may direct the steps of the humblest labourer, or a lucky thought may give the clue by which the mystery may be unravelled: this can alone support an adventurer such as I am, and be his only apology for venturing to meddle with this most difficult of all speculative subjects.

You will agree with me, that a theory of fever is wanted; one, at least, which might in some degree guide the practitioner; for even now, notwithstanding the many that have seen the light, we are forced to acknowledge that we grope in the dark, and to confess that our practice owes its success more to chance than to science. I know it to be the opinion of some that a rational and successful plan of treatment may be found independent of the knowledge of the proximate cause of fever. In this opinion, however, I cannot coincide; for, although we be successful in our practice, and therefore call our plan rational, we but combat a symptom as it appears with the remedies with which experience has armed us, and we anticipate another which experience teaches us to expect, but we know not what gives birth to the one, nor how the action we induce, operates in preventing the other. A knowledge of the origin of the phenomena of fever cannot be prejudicial to the practitioner; neither can an attempt to investigate it render him who makes it less skilful in his art. Perhaps our physiological knowledge has not arrived at that perfection which might authorize us to pronounce upon the functional derangements which give rise to symptoms, yet I think careful observation, induction from physiological facts, a proper application of pathological research and experiment, and a judicious culling from the theories extant, may yield principles approaching to truth, and convey to us some idea of the nature of fever. Cullen put so much value on the knowledge of the proximate causes of diseases, that he believed the cure to be founded chiefly, and almost unavoidably, on that knowledge; and it was with a view to the better conducting of the practice in fevers that he reared his theory, a structure which, as Dr. Good remarks, "if it be at this moment crumbling into decay, certainly is not falling prostrate before any fabric of more substantial materials, or more elegant architecture."

Every unbiassed physician believes with Cullen that the phenomena of fever in its earliest stage depend on some remote cause acting immediately on the nervous system; and, if the symptoms themselves did not point to this origin, the very sudden invasion of fevers, in many cases, is sufficient to warrant the conclusion.

The remote causes are certain sedative powers which diminish the energy of the brain. Though there be yet much to learn

regarding the functions of the brain, I think, if we pursue our inquiry with the knowledge we do possess, we may arrive at something satisfactory. For the sake of distinctness, let me explain what I understand by the energy of the brain, or rather in what sense I wish to be understood. By energy, I understand activity, a mere quality. By nervous influence, I mean the nervous fluid secreted by the brain and spinal marrow, and emanating from them. It appears that there are modifications of this influence, flowing by separate and distinct nerves; or, if not, the nerves themselves are so modified that, carrying the same matter, they perform separate and distinct functions. Such a modification was at first suspected, and is now proved by experiment; one set of nerves producing the effect of sensation, another set the effect of motion. But there is another modification, it is strongly suspected, either of nerve or nervous influence, which is supposed to play an important part in the animal economy, which, were it substantiated by experiment or otherwise, would, I think, enable us to thread the pathology of fever; and, in my opinion, the arguments in favour of such a modification are of more weight than those opposed to it. It is that modification of nerve or nervous influence which is supposed to effect secretion. The nervous hypothesis of secretion appears to me to be the only plausible explanation of this operation,—the only one which can be fairly and legitimately adopted, and, indeed, the objections brought forward by its opponents are attempts made rather to weaken the inferences drawn in its support, than facts directly opposed to it. Having said thus much, I think it incumbent on me to advert to some of those objections, and I trust you will pardon the digression, not only because secretion is a subject in itself highly interesting, but because, unless we can form some idea of the real nature of the function, we may bid adieu to the hope of tracing the proximate causes of idiopathic diseases.

Dr. Good concludes, with Sir E. Home, that, "the organs of secretion are principally made up of arteries and veins, but there is nothing in the different modes in which these vessels ramify, that can in any way account for the changes in the blood, out of which secretions arise." These organs being largely supplied with twigs of small nerves, naturally gave rise to the idea that through their instrumentality is secretion effected. In support of this view, Sir E. Home observes, "that, in fishes which are capable of secreting the electrical fluid, the nerves connected with the electrical organs exceed those that go to all the other parts of the fish, in the proportion of twenty to one;" and, in confirmation of this view of the subject, Dr. Good remarks, "that there are no parts of the body more manifestly affected, and few so much so, as the secretory organs, by mental emotions. The whole surface of the skin is sometimes bedewed with drops of sweat, and even of blood, by a sudden paroxysm of agony of mind; grief fills

the eyes with tears; fear is well known to be a powerful stimulant to the kidneys, and very generally to the alvine canal; anger gives an additional flow, perhaps an additional acrimony, to the bile, and, if urged to violence, renders the saliva of some animals poisonous; and disappointed hope destroys the digestion, and turns the secreted fluids of the stomach acid." Although the above seems to prove that the secretory organs are chiefly influenced by the sensorial system, yet it is denied that secretion is effected by nervous influence, because Haller observed that the larger branches of the nerves seldom enter into the secretory organs, and seem purposely to avoid them; because the secernent glands have little sensibility; and because the secretions of plants, which have no nervous system, are as abundant and diversified, and as wonderful in every respect, as those of animals. In all this I do not see one valid objection: it is now known that there are two distinct classes of nerves, independent of the great sympathetic,—the motor nerves, which can have no necessary connexion with the secernent glands, do avoid them, and even that perhaps purposely; and, if the glands possess but little sensibility, although largely supplied with twigs of small nerves, it is evident that all these twigs cannot proceed from sentient nerves, and, as no other nerves are acknowledged but those arising from the brain, spinal marrow, and great sympathetic, it is evident they must proceed from the last mentioned; the more especially as branches from the great sympathetic can be traced to every gland and every secreting surface.

Plants are living structures to be sure, but they differ from animals, inasmuch as they exhibit neither signs of sensation nor of voluntary motion: consequently, we do not expect to find in them either nerves of sensation or of volition. They differ in another point of view, which ought to lead us to think that the operations in the vegetable kingdom are principally effected by external influence: the laboratory of the vegetable kingdom is external, whilst that of the animal kingdom is internal. In this respect, plants are indeed the inverted animals of Dr. Alston. Linneus has been blamed for too fondly searching after analogies by which he might approximate the vegetable to the animal system, but it would appear that we are possessed of a similar propensity, since we reject a plausible explanation of an operation carried on in the animal, because, forsooth, we cannot analogically explain a seemingly similar operation going on in the vegetable. That the sunbeam possesses rays endowed with a deoxidizing power, the analysis of light has revealed to us. By exposure to these rays, and to atmospheric action, is the sap modified in the leaf; it is then conveyed to the cells in the parenchyma, where, by further exposure, it is fitted for the purposes required of it; and as in the light plants produce oxygen gas, when in the dark they do not, we can only conceive that oxygen is separated in consequence of some new combination effected

through the agency of light. Thus light may effect the decompositions and recombinations necessary to the vegetable, whilst nervous influence performs these processes in the animal: hence vegetable secretion is influenced by locality and the seasons, animal secretion by whatever perceptibly operates on the nervous system.

Dr. Bostock's principal objection to the nervous hypothesis, and which, by the bye, he brings forward as a direct argument in opposition to it, is, that secretion appeared to be produced in fœtuses which had no nervous system. I have no opportunity of examining the cases cited, but, admitting that in all of them the brain, cerebellum, and spinal marrow were wanting, it does not follow there was no nervous system, for in similar cases the sympathetic nerves have been found perfect; and, if the great sympathetic has more than one function, I am inclined to believe that secretion in general is performed through its instrumentality. Organs so important as the brain and spinal marrow are protected from external injury by their bony casements, but the sympathetic, although it has no such covering, yet, from its situation, it is still better protected, and, as nature does nothing without an object, we might infer that its functions are highly important. And what function, the due performance of which is more necessary to health and life than secretion? Sensation or voluntary motion may be impaired or almost wholly destroyed without necessarily causing death, yet these are functions performed through the instrumentality of separate and distinct nerves. Secretion is a function which cannot even in part be destroyed without endangering life, yet we deny that this function is performed through nervous agency, although we can trace nerves proceeding to the organs of secretion, knowing at the same time that they are nerves neither of sensation, nor of voluntary nor involuntary motion.

I have said that, by nervous influence, I mean the nervous fluid secreted by the brain and spinal marrow; but, by saying so, I do not wish to be deprived of the privilege of supposing that nervous fluid may not be secreted by the great sympathetic, and emanate from it also, for I do not think that Bichat was altogether incorrect in supposing that this nerve is independent of the great system of nerves. Anatomists do not say that the sympathetic arises from the sixth pair; for it is supposed that, instead of receiving filaments, it gives filaments to that pair. Or, according to Shaw, "when the foramen caroticum is opened, a plexus of nerves will be found surrounding the carotid artery, which appear to be united with the sixth; but which, when carefully traced, will be found to pass over the sixth, to unite with the cæsserian ganglion of the fifth. There will also be branches seen passing along the vidian nerve, to form in union with it the ganglion of Meckel." From repeated dissections, Mr. Shaw concludes that the fifth pair resembles the spinal nerves in every respect;

and by the fact that many of the branches of the sympathetic go to the ganglionic portion of the fifth, it is proved, he says, that even the connexion between the sympathetic and fifth is *similar* to the union of the sympathetic with the ganglionic roots of the spinal nerves. Notwithstanding this similarity of connexion, Mr. Shaw, it will be observed, in speaking of the branches of the sympathetic which join the ganglion of the fifth, constantly says that they go to the ganglion; whereas, in speaking of the spinal nerves, he says, "we shall find that each nerve has a double root,—i. e. one from the anterior and the other from the posterior column of the spinal marrow; that the one from the posterior has, immediately before it joins with the anterior, a ganglion formed upon it; and, if we carefully examine this, we shall find that from each ganglion a small nerve is *sent off* to join with the sympathetic."* The mode in which the sympathetic proceeds to and joins the casserian ganglion evidently proves that we are correct in saying that branches pass over the sixth pair to unite with this ganglion; and consequently, in describing the branches which pass off from the casserian ganglion, Mr. Shaw takes no notice of the sympathetic; whereas, since he has proved the union of the sympathetic with the ganglionic roots of the spinal nerves to be similar to the connexion between the sympathetic and the fifth, we certainly ought to believe, from analogy alone, that the sympathetic rather gives than receives a branch from the ganglionic roots.

The idea that the sympathetic is an independent nerve is almost confirmed by the observation that it has been found perfect in monsters in which the brain and spinal marrow have been found deficient; but this, it is said, forms no argument for its independency, for in such creatures the spinal nerves are also found, which are acknowledged to have their origin from the spinal marrow.†

This, however, appears to me not only to form no argument against the sympathetic being an isolated nerve, but to form one in favour of its being instrumental in assimilation. Mr. Shaw, who opposes the inferences drawn from the effects which follow the division of the par vagum, says that the same deductions have been drawn from experiments where the par vagum and sympathetic have been cut, as from those where only the par vagum was divided. This, however, so far from weakening the inferences drawn in regard to the par vagum, gives us cause to impute a similar power to the sympathetic,—viz. that it also is a nerve of secretion. If the stomach depended altogether on the sympathetic system of nerves for its supply of nervous influence, health, nay life itself, could be but the tenure of a moment, liable to be extinguished even by the ingesta meant to support it. But, to

guard against this, the stomach has been provided with a pair of nerves derived from the brain, through which, when a supply of gastric fluid is required, is the influence necessary to its secretion transmitted. Were it not for those nerves, the demand for nervous influence must have been made on the sympathetic system, and the supply granted at the expense of other vital organs. Even as it is, when nervous energy is very low, danger constantly attends a sudden call for nervous influence. A full meal to a person reduced to the last extreme by hunger is followed by death, because, as the brain cannot immediately supply the necessary influence, the ganglionic system is drained for the purpose, and the heart ceases to pulsate: in like manner, a cathartic one, reduced by disease, causes death by determining to the gut the little of influence with which the ganglionic system is then supplied. In fishes, the great sympathetic is extremely slender, but, as if to compensate for this, the eighth pair is remarkable for its size and distribution: a branch of it even extends under the skin as far as the tail fin,* evidently for the purpose of effecting the secretion of that slimy fluid which protects the skin. In the skate, that slimy fluid issues from ducts which terminate on almost the whole surface of the skin. These ducts Dr. Monro has traced from central parts, from whence they radiate, and there a pair of nerves terminate, which, before they divide, are nearly as large as the optic.† For what purpose, then, are such large nerves sent to these ducts, if it be not to effect the secretion of their contents.

Some of the oppositionists to the nervous hypothesis not only deny that the nerves are concerned in the process of secretion, but they would have us to believe that in some instances nervous action retards and disturbs the process. Nervous action is found in some instances even to retard and disturb the assimilating process; for it is a matter of observation that in many cases of hemiplegia, where the nervous power is withdrawn or impaired, digestion goes on better than in ordinary health, and an obstinate ulcer has been found to heal quickly after the limb was struck with palsy.‡

Were I to search for facts as arguments in favour of the hypothesis, I should certainly draw upon the above, and infer that digestion depends upon nervous, inasmuch as, in many cases of hemiplegia, it goes better on than in ordinary health. We have no proof that, in many such cases, nervous influence is not secreted: we know, however, that it is not transmitted as usual to the paralytic members, and therefore it is but reasonable to suppose that other parts of the body will be better supplied. Upon the same principle, opiate frictions

* Manual of Anatomy, by John Shaw.

† Shaw on the Superadded Nerves, (Lond. Med. and Phys. Journal, vol. 59.)

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* Outlines of Comparative Anatomy, by Andrew Fife.

† Ibid.

‡ Elements of Medical Logic, by Sir G. Blane, Bart.

improve digestion, and accelerate the healing of ulcers.

As there are at least grounds for supposing that secretion is a nervous process, we cannot help forming to ourselves some idea of the nature of that process; and, if our ideas do not actually militate against any known law of the system, there can be no harm in arranging them even on assumption.

Let us suppose the blood to be acted on by the nervous influence, after the manner in which fluids are acted on by galvanism, and we can conceive no apparatus more exquisitely fitted for the purpose of secretion than a gland. In a gland, the arteries soon become so minute that they cannot carry forward the grosser particles of the blood: these are returned by the veins, whilst the finer stream is carried towards the secretory duct; but in its passage there, and in its most extenuated state, it is subjected to the decomposing power of the numerous small nerves which proceed from the sympathetic; its elementary affinity is subverted, and the minuteness of the vessels which favoured the decomposition favours likewise the recombination; and, as the absence or presence of one atom is only necessary to change in toto the appearance and properties of matter, so we have, according to the force of the decomposing power, and the circumstances under which the recombination takes place, a diversity of fluids, differing more or less from the fluid furnishing the elements.

Yet, after all, the admission on all sides that the nerves possess an influence over the secretory organs, is sufficient for my purpose. The remote causes of fever being certain sedative powers which diminish the energy of the brain and nervous system, let us inquire what we ought to expect from such diminished energy. As the general condition both of body and mind depends upon the operative state of the brain, these must become enfeebled in all their powers, as a natural consequence of diminished energy of that organ. This enfeebled state is manifested by listlessness, dejection of spirits, and muscular debility.

But there are other symptoms, which our knowledge of the functions of the nervous system enables us to trace to the same cause. We know that the capillary arteries are under the influence of the brain, that they forward their contents by a power regulated by it; and the knowledge of this fact explains to us why paleness of the face, and cold sensations on the surface of the body, are invariably among the primary symptoms of fever. For, owing to the want of power in the vessels to carry on the circulation, the blood forsakes the surface: hence, were nervous influence present sufficient even for the evolution of heat, one material necessary to its production is in a great measure wanting. The diminished energy of the brain is sufficient in itself to account for the low quick pulse, but, as the heart is also influenced by its contents, we may there detect an additional cause of this symptom.

The change which the blood undergoes in the lungs is altogether depending on the nervous influence: if this influence, then, be not supplied in due force, that change cannot be duly effected, and the heart can only receive blood wanting in its stimulating principle. As all nerves require a regular supply of influence in order to the due fulfilment of their functions, all must be affected more or less when the energy of the brain becomes impaired; therefore the nerves supplying the respiratory apparatus share the effects. The diaphragm cannot contract as it ought to do, and the muscles destined to raise the ribs are unfitted for their office: hence the sense of weight or anxiety about the precordia, occasional sighing, or hurried breathing.

The particular sensation of cold of the back and pain of the loins may be referred to venous congestion, caused by the retrocession of blood from the surface pressing on the dorsal and lumbar twigs as they emerge from the spinal canal; for we know that pressure or irritation of a nerve in its course may give rise to unpleasant or painful sensations at its extremity, while the same pressure on the motor twigs must undoubtedly be the cause of the rigours. In the latter opinion I am, I think, in part borne out by the fact that, in those dying in the cold stage of ague, such congestion is found; and also by the observations of Bell and Shaw; for, when they irritated or simply pressed those twigs between the blades of the forceps, they observed that the muscles to which they proceeded were spasmodically affected.

As these and all other symptoms which manifest themselves during the cold stage of fever, may be traced directly or indirectly to impaired nervous energy, I shall, in the next number, pass to the second stage, or that of excitement.

From the London Medical Gazette.

A CASE OF NON-UNION OF THE THIGH,

*In which three Modes of Treatment were unsuccessfully employed; followed by a Description of the Appearances of the Bone, and Connecting Medium after Amputation; read by Mr. Amesbury before the London Medical Society, Feb. 18, 1828.**

Malcom Maclean, a strong healthy sailor, æt. 36, admitted into St. Thomas's Hospital, March 11, 1827, under Mr. Green, having a very loose oblique fracture at the upper part of the middle third of the thigh, of twenty-four weeks' standing. It having occurred on shipboard, with no medical attendant, nothing was done for ten days, when he was admitted into a Portuguese hospital at Monte-Video; and short splints, extending from the pelvis to

* Care has been taken to preserve all the points in the case; but, as read by Mr. A., it was too diffuse for our limited columns. We have, however, from the interest of the subject, devoted a considerable space to it.

the knee, were applied without, (after eight weeks' trial,) producing any good effect. Of course, directly after the accident, some pain, tumefaction, &c. arose, but, however, gradually subsided; and, at this time, (when he was removed on board to return to England,) all pain had left him, and the fracture was very mobile. Similar attempts at producing union were continued on the voyage home, but no good resulted.

A week after admission into St. Thomas's, Mr. Green very politely offered Mr. Amesbury the management of the case. On attentive examination the fracture was found (in the situation above described) oblique, extending downwards and outwards, very loose and mobile; by extension the ends could be brought into apposition, but otherwise the lower portion was drawn upwards and inwards, about two inches and a half; the upper somewhat bent upon the pelvis. The fractured ends could be freely moved on each other, (the lower portion could indeed be almost bent upon the upper,) without or with very trifling pain; some, however, was produced by firm pressure with the fingers.

First Mode of Treatment—By Pressure and Rest.—Mr. Amesbury having never yet failed in curing cases of non-union by this method, with the consent of Mr. G., determined to give it a fair trial; but stated at the time, his fears as to the result, not having before seen a case in which the fractured ends were so loose; and *suspecting*, as he did, that a preternatural* capsule had formed, which must, he suspected, be absorbed before callus would be thrown out. His apparatus, "which he has invented for fractures of the middle and lower thirds of thigh—simple and compound, fractures of the leg, and dislocations of the ankle," was applied, so as to *maintain "the proper length of the limb, and press the fractured surfaces strongly together."*

The apparatus was worn ten weeks; during the whole of which period more or less pain was felt in the seat of fracture, (except for three or four days before the removal of the apparatus.) The first few days it was severe, then becoming more moderate, the straps of the apparatus were tightened, and the pain again increased.

Although these symptoms, which precede union, existed, and were indeed more severe than in any former case Mr. A. had seen, no benefit was produced; the only effect being a condensation or adhesion of the soft parts around the fracture, and thereby, in some measure, confining them; but the mobility continued.

Treatment by Seton.—The opinion previously entertained of the existence of a preternatural capsule, probably lubricated by a synovial-like fluid, being now more strength-

ened, it was determined to introduce a seton between the fractured ends, which would not only destroy the integrity of the capsule, supposing it to exist, but also (and at all events) "produce a high action" of the parts favourable to the union desired.

The man, previous to the operation, was placed on one of Mr. A.'s fracture-beds, which, from its construction, allowed of the limb being first secured of its natural length; and, while in that position, a long and somewhat curved seton needle was, without difficulty, passed from behind forwards between the fractured ends, the silk with which it was armed being permitted to remain. The foot and pelvis being fixed with the limbs on the double-inclined plane on the fracture-bed, (without any retraction having for a moment been permitted to take place after the operation,) a splint was applied along the outer side of the limb, and secured in such a manner as to press the fractured surfaces gently together, without pressing the surrounding soft parts.

A good deal of constitutional excitement followed this treatment; the local irritation was rather severe, suppuration took place in two or three days, large quantities of pus were discharged, and he began shortly to emaciate. The matter in about a fortnight began to burrow under the fascia, and so much did his health suffer, that it was thought necessary on the nineteenth day to remove the seton. An opening had been made for the discharge of the matter which had burrowed at the upper and inner part of the thigh, from which, for some time, matter was copiously discharged, but gradually diminished, and ultimately both it and the openings from the seton-needle healed. The only effect on the fracture was a farther condensation and hardening of the soft parts around it, "such as has been often mistaken for callus."

Generous diet, tonic medicine, and the removal of local irritation, soon restored his usual good health; and every possible care was taken to maintain the fractured ends in apposition, and at rest: this, however, was, to the letter, impracticable during the succeeding five weeks of suppuration, it being necessary to move the splint daily to clear away the matter. When the suppurative process had ceased, he was continued on the fracture-bed, with the whole thigh enveloped in short splints, to again maintain pressure and rest. This was persevered in for a month, but to no purpose—the non-union was still to be cured.

Operation of cutting down upon the ends of the Bone.—Some time after the last report it was determined to perform the above operation, (the poor fellow entreating that something might be done,) and having exposed the fracture, to proceed as appearances might warrant.

The limb being secured on a double-inclined plane to command the femoral muscles, Mr. Green made a crucial incision, commencing about the middle of the rectus, and carried through the belly of the vastus externus to the

* Mr. Amesbury used in his paper read to the Society the term "artificial" capsule. This, however, appears preferable, and was suggested by Dr. Clutterbuck.

posterior part of the thigh. The flap was dissected upwards, and the fractured end of the upper portion of the bone brought into view, when it was seen that the extremities of the bone were connected by a thick, dense capsule, resembling somewhat that of the hip-joint, the integrity of which Mr. G. next destroyed by the removal of a portion, when the inner surface was found smooth and shining; about half an inch of the upper portion of bone was next removed by a Hey's saw; it was covered with a ligamentous deposition. Nothing could be done with the lower portion of bone, which, lying somewhat to the inner side of the upper, could not be got at without extending the wound of the soft parts, which Mr. G. and the surgeons present considered objectionable. Two or three small vessels being secured, he was left on the fracture-bed, and extension maintained in the manner peculiar to it, without any splint on the thigh. The wound was lightly strapped with adhesive plaster, and covered by a poultice.

It may be worth remarking, that during the operation he made no complaint, except when the finger was passed into the capsule, (in order to ascertain its extent,) when he complained of considerable pain being produced.

Very trifling constitutional irritation followed this operation, so that on the following day he was allowed full diet. "The limb became moderately swollen at the seat of the operation, and suppuration was established on the third day; the greater part however of the cut surfaces united, and but a small opening remained at the upper and outer part of the wound for the escape of matter, some of which subsequently burrowed to the upper and inner part of the thigh, producing some excitement of the system, which, however, quickly subsided when the matter was evacuated by the lancet. On the tenth day pressure was again employed by padding a splint, placing it on the outer part of the thigh, and tightening it," in order to produce absorption of the adventitious matters at the fracture. This was managed so as not to interfere with the wound.

Seven weeks after the operation the wound was not completely cicatrized, but the wounds at the upper part of the thigh were healed, and it was now determined to make more powerful pressure; in order to effect which, in addition to the outer splint, a short one was applied, extending on the inner side from the knee to the wound. The web of a tourniquet was now passed round the limb, over the splint just below the fracture, and thus tightening to any extent could be produced and maintained.

This, however, produced no particular excitement, although continued for a fortnight, when the limb was examined, and found no better than on his admission. It was perfectly useless, indeed an incumbrance, and the poor fellow begged to have it removed, which in consultation was agreed to, and on Friday last the operation was performed by Mr. Green in

a very masterly and somewhat novel style. It was a flap operation, but instead of making as usual two angles, one at the anterior and another at the posterior part of the limb, he left but one angle of the integuments, and *that* anteriorly, with a *circular* incision posteriorly; so that instead of trusting for the accuracy of the adaptation of his flaps to the extent of integument left, being precisely the same on both sides, the circular incision posteriorly admitted of his adapting the angles anteriorly to each other, with the *certainty* that the whole flaps would be accurately adapted, whereas if an angle had been left behind, *that* also must have been looked to. The bone was sawn through just above the fracture, and about half an inch below the trochanter minor.

Examination of the Limb.—The bone when sawn through was not altered in texture, as it will be remembered was the case with the portion removed at a former operation. The preternatural capsule was again entire, although a portion had been before removed; the greater part was thick and dense, but the upper portion was more thin: its internal surface was smooth, shining, and lubricated with synovial-like fluid. The ends of the bone, (the whole surfaces of which were not exactly in apposition, the lower portion being drawn to the inner side) were rounded, (except being flattened where they came in contact,) and covered with a dense fibro-cartilaginous structure, somewhat resembling the intervertebral substance, "especially that part of it found half way between the centre and circumference." The whole cellular tissue around was considerably condensed, and when cut presented some hardness.

Mr. Amesbury then read some remarks on the case, which our limits oblige us to notice very briefly.

That this patient is of a strong constitution, Mr. A. remarked, we have abundant evidence; and this accords with his observations in forty-five cases of non-union, so that it would appear that weakness cannot account for the occurrence. It is, however, pretty evident that the case has depended more on the want of proper measures being employed; and he contended that the short splints were ill calculated to produce the effect desired.

Mr. A. is now acquainted with *eighteen cases* of non-union,* in which the method he recommends, *pressure and rest*, has been adopted with success; but it appears there are excep-

* Most of these cases have existed six months; one nine, one ten, one eleven, and two fourteen months: sixteen of these cases were under his own care; and *none* that were so, were under treatment more than *ten weeks*, and *by far the greater number* not more, and several less than, *a month*. He has only known *two* cases of failure, the present case being one. Of twenty-four other cases he has *seen*, but not *treated*, in only *one* case has bony union been effected, and *that* after several months' suffering.

tions, and those of great importance to this success, viz. where a preternatural capsule has formed; and this may be suspected where there is great mobility of the fractured ends. In all cases, after a short (if unsuccessful) trial of his method, he recommends cutting down upon and removing with the knife any ligamentous matter with which the bones may be covered, and then very carefully washing the ends of the bones with some stimulating wash; this was indeed what Mr. Green had intended to do in the above case, but owing to the difficulties presented, he was unable to accomplish his object. It will afterwards be indispensable that great care be used in the mechanical contrivances, and he would submit these important considerations not only to the Society, but the profession at large, whether where preternatural joints have formed, the removal of the adventitious matters, the careful stimulation of the ends of the fracture, and judicious mechanical contrivances, will not cure these hitherto unfortunate cases.

From the London Medical Gazette.

VARICOSE ARTERIES.

We copy the following case from the *Glasgow Medical Journal*, the first number of which has just appeared, and which, from the zeal and acquirements of its editor, Mr. Mackenzie, and the ample opportunities for observation and experience possessed by the profession in the mercantile capital of Scotland, we have reason to expect, will prove an important addition to medical literature.

We shall subjoin the case in the words of Dr. MacLachlan, to whom it occurred, and then offer some remarks.

"Having caused that side of the head to be shaved, the better to observe the nature of the tumour, it presented the following appearances:—Soft, puffy, pulsating, and somewhat elastic swellings of a varicose appearance were found to occupy the course of the temporal, posterior auris, and occipital arteries and their principal branches, each branch terminating by a tortuous extremity. These swellings could be made partly to disappear on pressure, but on its removal they speedily regained their former volume. They pulsated throughout their whole extent, and the pulsations were synchronous with those of the heart. By pressing on the common carotid, the pulsations ceased all along the swellings; and, by intercepting the flow of blood through the temporal or posterior auris, the throb was interrupted in corresponding parts of the tumour. They were not painful on being handled, but he complained much of the torture he had experienced for the last two months, from the throbbing, which often deprived him of rest for nights together, and, as he said, made his existence miserable to him. The integuments covering the swellings were of their natural colour; only at those points which were most

prominent, they had a slightly bluish-red tinge.

"This *arborescent* tumour commenced in front of the ear, immediately over the zygoma, and quickly swelling out, it became of the size of a split lemon, lying transversely over the ear. It sent a process forwards on the forehead, communicating by a tortuous extremity with the supraorbital twig from the internal carotid; a large process upwards to the crown of the head; and backwards, the main body of the tumour communicated with the puffy swellings of the posterior auris and occipitalis, which latter vessels gave a varicose feeling to the scalp over the left side of the occiput.

"The largest and most prominent part of the tumour was immediately over the ear: at this point, the throbbing was very violent, and the integuments being very thin and rather pointing, it threatened ere long to burst.

"The history he gave of his disease was the following:—About ten years previously he had the temporal artery opened for an attack of ophthalmia. A small aneurismal tumour formed at the point of incision, for the cure of which the artery was cut across, lower down; but this not succeeding, the vessel was again exposed and a ligature applied. The little tumour disappeared, he says, only for a time; on its return it was but small, gave him no uneasiness, and although he served as a soldier for five years afterwards, he never complained of it to his surgeon.

"This disease seemed to me different from any kind of aneurism by anastomosis that I had either seen or read of. It evidently followed the ramifications of particular arteries, for, by pressure being made on a particular vessel, a corresponding portion of the tumour became flaccid and pulseless, showing distinctly that no free intercourse by means of cells existed in it.

"I proposed the trial of pressure: he said it had already been employed, and that it gave him so much pain that he would not again submit to it. He urged me at once to proceed to tie the carotid artery, as he was informed that that was the only means by which the disease could be effectually cured. I then explained to him the possibility of taking up the vessels singly; and that, should we fail, it was then in our power to tie the common carotid. He agreed—and with the assistance of Professor Towers and Dr. Anderson, I began by laying bare the temporal artery as it emerges from the parotid gland; but on dividing the fascia-like substance which kept it in situ, it shot forth through the opening in the form of a loop, in caliber larger than a goose quill; thinner in its coats, and, if possible, more diaphanous than a vein; and thrilling violently at each pulsation. A ligature was applied to this loop; the wound was brought together with adhesive plaster, and, for additional security, a firm compress and bandage were applied. It was now evident that the vessel was diseased at the point of ligature, and the propriety of tying the common carotid hence became obvious. Pulsation had,

however, ceased in the anterior and central portions of the tumour, which felt flaccid and doughy, showing that this plan of treatment would, in all probability, have been successful, so far as the vessels of the scalp, at least, were concerned, could the state of the arteries have been trusted to.

"Next day, the common carotid was tied, in the presence, and with the concurrence of Professors Burns and Towers, Drs. King and Anderson. An incision, about two inches and a half in length, was made along the inner edge of the sterno-cleido-mastoid muscle, commencing at the lower edge of the thyroid cartilage, and extending downward to within half an inch of the sternal extremity of the clavicle. A large branch of the exterior jugular vein ran across the line of incision, but by carefully cutting through the fascia-like platysma myoides, the vessel was easily drawn aside along with that muscle. The sterno-cleido-mastoideus was now seen forming the outer margin of the wound, and the omo-hyoideus crossing it superiorly. The dissection was cautiously carried deeper, until the descendens noni was seen over the sheath of the vessels. The artery and par vagum were now distinctly in view, but the internal jugular vein, which frequently gives much trouble during this operation, did not at all appear. The sheath was opened by cautiously scratching with the point of the scissors between the carotid and par vagum, which nerve was carefully drawn aside. A blunt aneurismal needle, armed with a very fine silk ligature, was now introduced, and passed with ease, from without inwards, under the vessel. The ligature being tied, the ends were cut short, and the artery left undivided. The wound was brought together with sticking plaster, and a light compress and bandage applied. He did not lose above a spoonful of blood, and his pulse, immediately after the operation, was 78, and of good strength. Immediately on tying the vessel, the varicose tumours of the head became devoid of pulsation and felt flaccid, although their prominence was but little diminished.

"In the evening he felt his neck rather stiff—had slight headach, seated principally under the *right* temple. Took some food with relish. Pulse 84. Skin rather hot.

11th July.—He passed a good night. Wound free from pain. Pulse 92. But in the course of the forenoon he was seized with severe pain of chest, particularly in the right side. Pulse 120, of moderate strength. Skin hot. Tongue white. No stool. *V. S. at two bleedings to 70 oz.; saline purgatives; a blister; blood first drawn much buffed.*

"12th.—A bad night. Pain of chest easier, but breathing much oppressed, from a sense of weight over the chest. Pulse 108, soft. Skin nearly natural. Two copious stools. Wound feels quite easy. *Tinct. digit. and anodyne at bed time.*

"13th.—A good night, having slept for five hours. Respiration and sense of oppression as yesterday. Towards the afternoon

complained of pain in the region of the liver, increased on pressure. Pulse 116, rather full. No stool. *V. S. to 24 oz.; castor oil; turpentine enema; warm fomentations; anodyne at bed time.*

"14th.—Dozed during the whole night, and now mutters almost constantly, but he is quite collected when spoken to. Said the pain of liver was easier. Respiration much oppressed. Pulse 144, feeble. Perspires copiously. No fæces passed with the enema. He became gradually worse, and died at 5 P. M.

"*Dissection*, fifty hours after death, in presence of Dr. Anderson and several medical gentlemen of Paisley, to which town the body had been removed. The weather being very hot, putrefaction had made considerable progress. The viscera of the abdomen appeared healthy. The intestines were much distended with flatus, devoid of fæces, and blanched.

"In the chest, some straw coloured puriform matter was found in the anterior mediastinum: about a pint of thick grayish mucopurulent matter in the right cavity of the pleura, and a small quantity of bloody extravasation into the left. The pericardium was unusually devoid of fluid—the heart large and flaccid.

"The wound, which had adhered throughout its whole extent by the first intention, had partially re-opened from putrefaction. The carotid, the par vagum, and jugular vein appeared as if they never had been disturbed, nor was there the least appearance of pus around the ligature. On slitting up the artery, and cutting through the ligature at the same time, (the ligatured portion having been previously removed from the body,) small but soft clots were found above and below the ligature, and the artery remained puckered from the recent deposition of lymph. Its inner coats were divided as with a knife, while its external was found dense, strong, and entire. Below the ligature, the inner coat of the artery was of a vermilion red colour, even that of the thoracic aorta bore equal marks of inflammation, but at the bifurcation it was of its natural aspect; that portion also of the aortic arch nearer the heart than the coming off of the left carotid was healthy. The carotid in the neck was of its usual size, strength, and thickness; but on examining its branches on the head, they were found to have degenerated into dilated tubes of extreme thinness and transparency; which, apparently, yielding to the impetus of the blood, had become elongated, contorted, and ultimately convoluted on themselves, so as to form, by this species of doubling, the tumours which constituted this singular disease. These tumours felt like placenta, and to the eye, the larger portion immediately over the ear looked precisely like a bundle of earth worms coiled together.

"I regret that from the peculiar circumstances under which this inspection was obtained, I had not an opportunity of examining, more at leisure, this very unusual disease of the arterial system, or even of ascertaining the

exact point at which the disorganization commenced; whether the artery became gradually thinner, or whether the disease began suddenly; whether in the diseased portions the three coats existed, or the dense, but thin external one only remained; whether the branches of the internal carotid were similarly affected with the external, and thus giving rise to the epileptic fits to which he had recently become subject: these points unfortunately must be left to conjecture.

"I have been at some pains in searching through books for analogous cases, but Pelletan is the only author, as far as I know, who has distinctly described this disease. In his *Clinique Chirurgicale*, tome ii. two cases are given, which coincide in every particular with the one here detailed. One of these only, a girl 18 years of age, he had an opportunity of treating. Compression was first tried, but the patient could not bear it. He then tied the temporal artery: this promised to be successful, as far as the portion of the tumour supplied by that vessel was concerned, when unexpectedly the patient died, in consequence of an 'indigestion.' He has fortunately favoured us with plates of this case, which are of great assistance in elucidating this subject, for he has said but little pathologically of the nature of the disease. He speaks merely of dilatation, but it is evident from plate ii. fig. 2, in which the convolutions of the arteries of the scalp are given, on dissection, that the view above advanced, viz. the doubling of the dilated vessel on itself, as the cause of the tumour, is a correct one. Boyer,* also, who saw this case, and has given the dissection more at length, says, in speaking of the structure of the tumour—"Toutes les artères comprises dans la tumeur, au dessous du tissu donc nous venons de parler, étaient dilatées, flexueuses, bosselées, ici très larges, là très étroites, pleines de sang caillé, ou d'une humeur blanche et épaisse. L'artère temporale depuis son origine, jusque vers le milieu de la tempe, avait éprouvé une simple dilatation Plus haut l'artère temporale, et ses diverses branches, étaient bosselées, flexueuses, grosses, et rouges." His remarks on the occipital branches are to the same effect.

"Mr. John Bell describes aneurism by anastomosis, to which the disease under consideration has most affinity, to be 'a congeries of active arteries absorbing veins and intermediate cells.' Now, in the tumour which I have attempted to describe, there were no cells, no parenchyma as in the spleen, the bulk of the tumour was formed almost entirely by convoluted dilated arterial trunks, the veins being but little changed from their healthy state. These arteries did not appear to communicate more freely than by their ordinary inosculation; and in the less prominent parts of the swelling, they had more of the appearance of the contorted vessels of the gravid uterus, as represented in Tiedeman's beautiful plate, than any other anatomical comparison I can give.

Mr. Abernethy* evidently alludes to this disease when he says, while speaking of nævus, 'for this preternatural enlargement of vessels is not always cutaneous. I have seen it occupying the whole substance of the cheek, neither appearing beneath the skin nor the membrane of the mouth. I have seen it in the orbit,' &c. The cases reported by Messrs. Travers and Dalrymple, in the *London Med. Chur. Trans.* vol. ii. and vi. in which the carotid artery was tied for pulsating tumours of the orbit, appear to have been of this description.

"I have made these remarks and extracts because I conceive there exists a pulsating tumour composed entirely of dilated and convoluted arteries, whose inosculation and interlacings are not more numerous than usual, only they become more apparent from their increased size. That this tumour may occupy a great extent of surface, such as the side of the head, the neck, or arm, from its following in a continuous manner the course of the arteries. That the term aneurism by anastomosis is not very applicable to it; and that from the advanced age of many of the persons in whom this disease has been met with, its congenital nature is more than doubtful, although Boyer, Pelletan, and others are of this opinion. Whether, in the case of Maclure, the vessels were perfectly sound at the period of opening the temporal artery, may admit a doubt. The difficulty of curing the small aneurismal tumour favours this doubt: yet there certainly existed no obvious trace of enlargement of these vessels to lead to this conclusion. In explanation of this remark it may be necessary to state that the artery was opened by myself. The opening of the temporal artery, however, probably operated as an exciting cause on vessels already disposed to disease."

The above case is very interesting, more especially at the present moment, in connexion with that which was operated upon by Mr. Wardrop some little time ago†—and which was recently the subject of a post mortem examination at the Middlesex Hospital. These two cases, with that related by Pelletan in his *Clinique Chirurgicale*, are instances of the same diseased state, consisting of the dilatation of one or more branches of an arterial trunk, not in any respect like that of aneurism, or limited to one point of the vessel, but occupying the whole extent of a branch, or all the branches of one trunk. It resembles, in this respect, the diseased condition of the venous system constituting varicose veins, and also in the following circumstances, that the artery or arteries, as they dilate, progressively lengthen and form convolutions coiling upon themselves, and presenting externally an irregular tumour, not unlike (with the exception of pulsation) that of a varicose vein. This resemblance is such, and the appearance of an artery so diseased, with reference to that of a healthy

* Surgical Works, vol. ii. p. 225.

† Vide Journal of Foreign Medicine for February, p. 152.

* Traité des Mal. Chir. tome ii. p. 293.

artery, is so analogous to that of a varicose vein to a healthy vein, that we have been led to apply the term *VARICOSE ARTERY*, to designate the disease in question: a name which seems to us nearly unexceptionable, being sufficiently characteristic, and conveying a very definite and intelligible idea of the nature of the affection in contradistinction to that of "pulsating tumour," which has been applied to it.* As to the difference of varicose artery from what has been described as aneurism by anastomosis, the former is a disease seated in the middle-sized trunks, and in the branches of the arterial system, the latter is a disease of its capillary extremities; what the one disease consists in is evident to the senses, and has been the subject of examination; what constitutes the other we are ignorant, or at least it is still a matter merely of surmise.

In regard to the treatment of varicose arteries, we have mentioned in what respect, and how essentially, this disease differs from aneurism, properly so called, and that by anastomosis; it is, therefore, not to be wondered at that the principles of treatment adopted in these should neither of them be found quite applicable. We have here no single sac, where, upon tying the vessel supplying it, the blood contained therein stagnates and coagulates, and the artery becomes obliterated at *this* point. On the other hand, we have an artery or arteries, dilated for a considerable extent of their course, where there is no distinct sac and no stagnation of blood, but where this fluid is constantly circulating, and where the only effect of the application of a ligature to the trunk supplying the diseased branches, is to interrupt for a time the circulation through them, but where no blood remaining in them to coagulate, no permanent obstacle to the circulation is formed; and the arteries, therefore, again become filled, and the disease resumes its activity so soon as the collateral circulation is established. With regard to excision of the diseased artery, there can be no doubt that this would be the most effectual mode of treatment, but it is obviously one which can seldom be applicable—from the great extent to which the disease has proceeded before relief is sought—from the few situations in which it would be practicable,—and, what is of still greater importance, from the difficulty, if not the impossibility, of determining where the diseased condition commences or terminates. In Pelletan's case, when he cut down upon the trunk of the temporal artery at the zygoma, with a view of tying it, and of course with no idea that it was otherwise than healthy, he distinctly states that he found it greatly dilated at this point; which circumstance is given by him as an apology for his having pierced it with the aneurism needle. It will be seen that Dr. Maclachlan likewise, unexpectedly, found the trunk of the temporal artery, at this point, to participate in the

morbid dilatation; and it is not clear to us that, even in the case of Nowlan, the trunk of the temporal artery was healthy, both from what occurred on the attempt at operation at this point, from the farther progress of the case, and from the appearances on dissection. It will be evident that the difficulty of previously ascertaining the real condition of the main *branch* in the vicinity of the disease, presents an equal objection to the treatment by ligature of the vessel which gives off the dilated branches.

These remarks, however, do not apply to the main *trunk* at a greater distance from the disease, as for instance to the carotid, which was found to be healthy in the cases of Mr. Wardrop and Dr. Maclachlan; but we have already given other reasons for not anticipating a favourable result from a ligature applied to it.

From the Medico-Chirurgical Review.

Medico-Chirurgical Transactions.

REMARKABLE FUNGOUS ERUPTION CURABLE BY MERCURY. BY MR. WALLACE.

The unsightly disease which forms the subject of this communication appears to be much more frequent in the Sister Island than here. Mr. Wallace, who is attached to an institution for cutaneous diseases in Dublin, ventures to give it the name of *morula*, from *morus*, a mulberry. This term marks its most prominent feature—"a fungus of a rounded and granulated form." Although its appearances are regular and peculiar, it has not previously been described. Its growth is somewhat similar to that of the yaws; but there are differences in other respects, which forbid its classification with that disease. Our author has not had occasion to see the eruption, except among hospital patients—and it is curious that the majority of the individuals were either males or females, who got their livelihood by traffic in old clothes, rags, and similar merchandize. It has frequently been observed, however, by others among the peasantry of Ireland. Mr. W. does not think it has any necessary connexion with syphilis, nor pruriginous affections—nor, in short, with any other disease. On some occasions, there was reason to suspect that it was propagated by immediate contact—but generally it appeared void of contagious character. There is scarcely a part of the body, except the palms of the hands and soles of the feet, upon which Mr. W. has not observed these fungous eruptions.

"They uniformly commence, as far as my observation goes, in minute pimples, which become quickly covered on their apex by a very small scab, upon the removal of which may be observed the germ of the future fungus, consisting of a single granulation, and so minute as to require for its discovery the assistance of a lens. At this period the spot is itchy, and is surrounded by a slight erythematous redness. Its size gradually but pro

* The term varicose was applied to a vein merely from appearances, and without reference to the condition of its coat.

gressively increases. In the course of some days, a scab of several lines in breadth, of a brownish yellow colour, and considerably elevated, will be found to cover a fungus of a rounded figure and granulated surface, of a yellowish red colour, sore to the touch, and surrounded by a slight livid redness. The size to which these spots are capable of increasing as fungi is, I believe, limited. I have never observed them larger than about one inch and a quarter in diameter, and I have generally remarked that when the spot acquired about an inch in diameter, the action of the vessels of the part changed, and the fungus becoming absorbed, an ulcer was produced. If credit could be given to the observation and reports of the patients themselves, it would appear that many of the fungi, upon arriving at a certain magnitude, shrink and fade away; but whether a fungus, after it has been once decidedly formed, ever disappears, except by the formation of an ulcer, without the interference of art, is a point upon which I cannot speak decidedly from my own observation."

It is very remarkable that the part on which these fungi have been situated, possesses the power of healing, without the formation of any permanent cicatrix—demonstrating that the fungi grow from the *surface* of the cutis, and that the texture of this covering is not permanently injured. The number of fungi in individuals, varies from one to fifty. The THERAPEUTICA of the subject will clearly appear in the following extract.

"In the whole catalogue of maladies which are capable of being cured or relieved by mercury, I do not know any which exhibits the value of this mineral more remarkably than the disease in question. I believe it matters nought whether this valuable agent be employed internally or externally. If internally, whether it be used as an oxide or salt; if externally, whether it be employed in the form of an unguent, vapour, or fluid, for the disease immediately shrinks, *as soon as the slightest mercurial action in the system is manifested*. I generally retain the patients under care five or six weeks, and use the remedy to such an extent as to cause its gentle but marked influence on the system; and the form in which I employ it is varied according to the peculiar circumstances or convenience of each case."

From the Medico-Chirurgical Review.

ON DISLOCATIONS OF THE VERTEBRÆ. BY W. LAWRENCE, F. R. S.

[*Medico Chirurgical Transactions.*]

Some of our best surgical authorities maintain that, with the exception of the first and second bones of the neck, complete dislocations of the vertebrae, without fracture, are nearly impossible. Boyer, Delpech, Sir Astley Cooper, may be cited on this side of the question. Other surgeons (and even physicians) have affirmed, that the said bones may be luxated. Rust says, that even the lumbar and dorsal vertebrae may be dislocated—and Ger-

man research has pointed out several recorded cases of this kind—but, whether true or false, is another question. One case Rust mentions as having been treated by himself. "The injury was produced by a severe fall on the head. The neck was bent completely to the right side, the upper extremities being paralyzed, attended with hiccup and convulsions. Replacement was immediately attempted, and succeeded. I made the patient sit on the ground, and had the head drawn straight upwards by a strong assistant. The patient got well under the employment of cold locally." Mr. Bell mentions a case of complete dislocation between the last dorsal and first lumbar vertebrae, with entire division of the spinal marrow—but a small portion of bone was broken off.

"The greater mobility (says Mr. Lawrence) of the individual bones, the comparative smallness of their bodies, and the obliquity of the articular processes, point out the cervical vertebrae as those most likely to be luxated; at the same time, the form of the neck, and its connexion with the head, are favourable to the application of such violence as may cause luxation. Hence not only does dislocation of the atlas occasionally occur, but we have also instances of luxated articular processes in the case of the five inferior cervical vertebrae. Baron Boyer even considers that this may happen without external violence, and that the inferior articular process of a cervical vertebra may be carried in front of the superior articular process of the vertebra below it, by a sudden and forcible rotation of the head and neck towards the opposite side. He says that, 'Desault mentioned in his lectures the case of an advocate, who met with this kind of dislocation, by turning his head suddenly round to see who was coming in at a door situated behind his seat. Chopart also showed us a young man, 24 years old, in whom a similar accident had occurred, in consequence of an extreme rotation of the head, leaving the head permanently inclined upon the left shoulder.'"

Sir Astley Cooper never saw an instance of the dislocation without fracture, but does not deny its possibility. In the anatomical museum of Bartholomew's Hospital, there are some specimens of this accident; but we proceed to Mr. Lawrence's own cases.

Case 1. C. B. aged 22, was brought into Bartholomew's Hospital on the 8th January, 1827. He had slipped while carrying a heavy barrel, and fell with the weight resting on the head and upper part of the back. He was instantly deprived of sensibility and motion in the trunk and limbs. In this state he was carried into hospital, all below the neck paralyzed, except the diaphragm. The chest was motionless—pulse weak—body cold—priapism. No irregularity was perceptible in the line of the spinous processes. He lived till the morning of the 12th, viz. better than three days, when he expired, apparently from interruption of the respiratory process.

Dissection. No external displacement could be detected in the dead body. But the fol-

lowing examination showed the nature of the injury.

"After cutting away the muscles from the back of the spine, the cartilaginous surfaces of the superior articular processes of the fifth cervical vertebra came into view: they were exposed in consequence of the inferior processes of the fourth vertebra having been completely dislocated forwards, and remaining fixed in their unnatural position. The yellow ligaments connecting the laminae of the two vertebrae (ligamenta subflava) were torn through, and the bifid apex of the fourth spinous process lay in close contact with the basis of the fifth. On the front of the column an unusual projection was observed, but the anterior longitudinal ligamentous expansion was entire. The body of the fourth was completely detached from that of the fifth vertebra, the connecting fibro-cartilage being torn through, and the body of the former projecting by its whole depth in front of the latter. In consequence of this displacement, the antero-posterior diameter of the vertebral canal is lessened by about one-third. The section of the bone was not made till some days after death, so that the recent state of the spinal marrow could not be estimated."

The following curious case happened in the practice of Mr. Wigan, and the preparation was shown to the Society with that of the former case.

CASE 2. A child, at the age of five or seven years became affected with an illness supposed to be hydrocephalus, and, after some time, a swelling took place on the side of the neck, containing obviously a fluid, which increased to a considerable size. Pressure on this affected the brain, and produced coma. It was, therefore, supposed that a communication existed between the fluid in the tumour and that in the head. After a long continuance, the tumour disappeared, together with the symptoms of the supposed hydrocephalus. There was no interruption or diminution of sensation or motion at any time, and the child became active and lively. After a time, disease came on in the lumbar vertebrae, attended with bending forwards of the spine, and the formation of a large lumbar abscess, under which the little patient sunk at the age of 12 years.

The head was examined in hot weather, and the brain had become so soft that the changes produced in it by disease could not be ascertained. Mr. Wigan brought Mr. Lawrence the base of the skull, and they were surprised to see a considerable bony prominence standing up in the right side and front of the foramen magnum. This projection was smoothly covered by the dura mater, and proved to be the dentiform process of the second vertebra. After maceration, they found an extensive displacement of the occiput, atlas, and axis, these bones being firmly consolidated in their new relative positions by bony ankylosis of several articulations. The atlas was partially dislocated towards the left; and, at the same

time, thrown a little forwards and upwards—hence the right and posterior part of its bony ring intercepted a considerable portion of the spinal canal.

"The middle anterior protuberance now corresponds to the left side of the basilar process; the extremity of the left transverse process projects three quarters of an inch beyond those of the two following vertebrae, while the right transverse processes of those vertebrae project one quarter of an inch beyond the corresponding one of the atlas. A considerable part of the right side of this bone has been destroyed by absorption: that is, the surfaces by which it is articulated to the occiput and atlas, a part of the transverse process, and that groove on which the right vertebral artery rested. The axis is completely dislocated from the atlas and occiput to the right, so that its left portion intercepts about one-third of the spinal canal, and the dentiform process projects by its whole length, into the cavity of the skull at the anterior part of the foramen magnum, close to the right anterior condyloid foramen."

The state of parts cannot, indeed, be well understood without seeing the preparation or the drawing; but it is astonishing, says Mr. L. that the immediate pressure of the bony projection on the under surface of the medulla oblongata, caused no paralytic affection—"even when we allow for the very gradual manner in which it must have been produced." Mr. Lawrence will find, on searching the records of pathological facts, that this "gradual manner" of encroachment on vital organs is every thing. We see the brain itself reduced almost to a shell by the pressure of collected water in the ventricles, with scarcely a diminution of the physical or intellectual functions of the sensorium—why then should we wonder that the medulla oblongata accommodated itself to the slow growth of a bony projection in its neighbourhood?

In some interesting observations on diseases of the cervical vertebrae, Mr. Lawrence quotes a passage from a paper of Professor Rust, of Vienna, as published first in a German Journal, and afterwards enlarged in his work on Diseases of the Joints, where he states, that he saw thirteen cases of the complaint.

"Pain in the neck, becoming more severe at night, or in swallowing a large mouthful, or drawing a deep breath, is the first symptom. This pain affects one side of the neck, especially when the head is moved towards the shoulder; it extends from the larynx towards the nape, and often to the scapula of the pained side. No external alteration is perceptible; but firm pressure on the region of the first and second vertebrae produces considerable pain, and thus points out the seat of disease. The difficulty of swallowing and breathing, and hoarseness increase, alternating with pain in the neck, which seems to fix about the back of the head, and becomes intolerable on moving that part. The head sinks towards one shoulder, the face being turned a little down, for in general the articulations are affected on

one side only, and that was the left in seven out of nine examinations after death. If both sides are affected the head will incline directly forwards. In this state things continue for several weeks or months; and before worse symptoms come on, there is often apparent improvement, freer motion, and more natural situation of the head. But the uneasiness in speaking and swallowing returns, the pain becomes more severe and extensive, the head falls a little backwards and sinks towards the opposite side. The patient feels as if the head were too heavy, and he carefully supports it with his hands, when he moves from the sitting to the lying position, or vice versâ. This may be considered a pathognomonic symptom of the affection. Another symptom, which at this period shows the true nature of the disease, is a peculiar expression of pain in the countenance, which, combined with the position and stiffness of the head, constitutes so characteristic an assemblage of appearances, that it is enough to have seen it once, in order to recognize it again immediately. This look of the patient, which Rust has endeavoured to represent in an engraving, consists especially in a general alteration of the features, with heavy motion of the eyes, and a dull melancholy expression of internal painful sensations. More active indications of severe suffering are observed whenever the head is moved.

"In the further progress of the case, noise in the head, deafness, giddiness, cramps and convulsions, partial paralysis, particularly of the upper limbs, loss of voice, purulent expectoration, and hectic symptoms supervene. Generally, no external change is observable, either in the neck or in the nape; and Rust observed, in one case only, swelling of the affected side, which broke and left fistulous ulcers. But the slightest pressure in the region of the three upper vertebræ is acutely painful, and sometimes in the advanced period of the disease a grating of rough surfaces is distinctly perceptible when the head is turned. The patient may continue for months in this helpless and painful state, and then dies, either from exhaustion and debility, or, which is more frequent, suddenly and unexpectedly."

Such is Rust's description of a disease which Mr. Lawrence thinks we can have no hesitation in regarding as originally ulceration of the cartilages, proceeding to destruction of the ligaments, and caries of the bones, with extension of disease, in various shapes and degrees, to the neighbouring important parts. Several allusions are made to cases observed by Reil, Bell, and others, of this affection. Rust thinks the complaint must be almost always fatal—at least it proved so in nearly all his own cases.*

* Although we do not profess to be such "learned Thebans" as the author of this paper, yet we cannot but wonder that the work of Johannes Baptista Paletta,* the first volume of which was published in 1820, should

* *Exercitationes Pathologicæ*, 2 vols. 1820–1826. Milan and Paris.

In regard to the treatment, it is evident that the principles which are applicable to disease seated in other parts of the vertebral column, ought here to be put in force. On account of the proximity of great and important organs in this last disease, it is a matter of great consequence to lessen the degree and extent of inflammation, and thus limit the disorder in its early state. Local depletion and the other antiphlogistic plans—perfect quietude—and counter-irritation, are the principal means, by which we can hope to control this formidable complaint, when it is capable of control, or within the range of cure.

From the Edinburgh Medical and Surgical Journal.

REMARKS ON STRUMOUS CORNEITIS AND IRITIS, WITH CASES. By JAMES GUTHRIE, Esq. Surgeon, Kilmarnock.

The ophthalmia to which I have given the above designation, occurs most frequently in girls of a strumous habit, about the age of fourteen, in whom the menses have not appeared.

The inflammation is slow and insidious in its progress, marked by a very slight redness of the conjunctiva, and the greatest possible degree of intolerance. The vessels on the sclerotica are seen on close inspection very minutely injected, and running in straight lines to the margin of the cornea, where they form the well known appearance of a vascular zone. The cornea soon becomes opaque; the iris contracted; and the aqueous humour muddy. A profuse gush of scalding tears follows the opening of the eye. Though the sensibility is morbidly increased, the patient rarely complains of pain either in the eye or head, the aversion to light being the principal distress.

The constitutional symptoms run very high; the pulse becomes quick; the appetite is lost; and the bowels constipated or otherwise deranged; skin harsh and dry; and the whole system in a high state of irritation.

I have not been able to detect any very evident exciting cause. The disease occurs most frequently in the months of April, May, and June; and invariably in those of a strumous irritable habit. Girls are most liable to it, particularly about the age of fourteen. As an exception to this remark, I lately saw a well marked case in a woman forty years of age, in whom the menses were suppressed. The sight was irrecoverably lost before the patient consulted me. The case ended in a leucomatous state of the corneal texture, with opaque capsule, to which the pupillary margin of the iris adhered by a pencil of lymph.

have escaped his vigilant research. The 15th chapter of that work is dedicated to fractures and dislocations of the vertebræ, in which are recorded cases of every description, from the atlas down to the last lumbar vertebra. In the next Essay which Mr. Lawrence favours us with, he may draw on this work for very ample materials indeed.

The inflammation has its seat principally in the cornea and iris, both of which are affected simultaneously; and where the whole of the morbid action is expended, the other textures no doubt participate in the inflammation, particularly the choroid; and although the evidence on this point is rather deficient, its existence, I think, is indisputable.

The disease is extremely obstinate, running a course of from two to three months, yet, when well managed, rarely produces permanent injury to vision. The cornea, though apparently disorganized, resumes its transparency, and the iris its functions, under the influence of those remedies which subdue the inflammation and photophobia. But when allowed to run on uncontrolled, it terminates in coadhesion of the iris and cornea; constricted pupil; opaque capsule, with partial adhesion thereto of the pupillary margin of the iris. If the anterior chamber is preserved, a leucomatous state of the cornea is a frequent result.

Treatment. The mild, indolent character of the disease renders it more difficult to subdue than a fixed and vigorous action; and the practitioner is insensibly betrayed into irresolution, by the seeming inertness of the disease and the obvious delicacy of the patient. In this kind of ophthalmia I have never found the detraction of blood, either local or general, of any avail. The cases in which it was employed turned out the most obstinate and intractable. Leeches are not much less debilitating than the lancet, owing perhaps to the local irritation thereby induced. Syncope, in such cases, from dread of the lancet, is the best substitute for the detraction of blood. The principle of treatment indicated in such cases is to lessen the irritability of the system, without materially depressing its power. This object is most effectually obtained by the administration of emetics, which should be repeated every second or third day for the first week. We must, at the same time, exhibit ten or twelve grains of Dover's powder every night, to be followed up next morning by a small dose of sulphate of magnesia, to regulate the bowels and prevent the constipating effects of the opium.

This treatment will soon give a turn to the inflammation, and reduce the morbid irritability. This being accomplished, mercury must be had recourse to, and introduced into the system, so as to disturb it in the least possible degree. Profuse salivation is not wanted. Its effects are often very decisive long before the gums become affected. Mercury in this disease is a remedy of great value, and will seldom disappoint us, if not too early employed. I erred myself at one time in this particular. Until the irritability of the system is reduced by the means pointed out, mercury is injurious. This fact I have had many opportunities of establishing; but when seasonably employed I know of no remedy competent to the same effects. Blisters are not useful. The benefit expected from them is altogether counterbalanced by the irritation they produce.

Belladonna ranks next in importance to mercury; without it the case could not be brought to a favourable termination. The extract should be thickly painted on the eyebrow and eyelid every night at bed-time. Besides its influence over the pupil, which is very limited for some time, it has a considerable effect in allaying the local irritability.

Stimulating drops and ointments must be discarded altogether during the acute stage, as well as every other doubtful remedy. The only local application from which I have seen any benefit, is the steam of warm water containing laudanum. From this the patient generally derives great relief.

When the inflammation has been considerably reduced, and the intolerance of light removed, a little weak precipitate ointment introduced into the eyes twice a week, will be of great service in restoring the cornea to transparency. Solutions of nitrate of silver, or muriate of mercury, or vinum opii may be occasionally interposed.

In the convalescent state tonics and sea air are required to complete the cure and re-establish the health. Should the disease lapse into a chronic form, a termination which I have occasionally seen, caustic issues superadded to the local treatment will be found successful.

I now proceed to the detail of two cases. I have selected the following as being most illustrative of the foregoing observations.

Case I.—Margaret G. aged 14, of a strumous habit and delicate constitution, has been affected with inflammation in the left eye for a fortnight. The symptoms are the following: Redness of the eyeball apparently trifling; close inspection, however, discovers the vessels on the sclerotica very minutely injected, and running in radii to the margin of the cornea, where they form a zonular appearance. The cornea is hazy, and its serous vessels loaded; iris contracted; intolerance of light excessive; lachrymation, and vision is much impaired. She makes no complaint of pain either in eye or head; pulse small and quick; and the constitutional disturbance very considerable. Menstruation has not commenced.

The following treatment was in the first instance adopted: leeches repeatedly applied to the temple, and a suppurating surface established behind the ear. The bowels being cleared by a purgative, calomel in combination with opium was given twice daily. A solution of nitrate of silver was applied to the eye, and extract of belladonna to the eyelid every night. The above treatment was persevered in for four weeks, without in the least interrupting the progress of the disease. The mercury had in the interval affected the gums, and was given up. The cornea is now rendered opaque, and very much thickened, presenting in one spot a circumscribed deposit of lymph. At this part the cornea has an elevated appearance. Iris extremely contracted and irregular, but with difficulty seen; aqueous humour muddy. The pupil resists the influence of the belladonna; aversion to

light unabated; vision gone. The right eye has taken on the same morbid action. The constitutional disturbance is now so great as to confine the patient to bed. The irritation in the system (I mean its sympathy with the local disease,) appears to play the most important part in keeping up the disease; to subdue which, emetics every second day, Dover's powders in doses of ten grains every night, and half an ounce of Epsom salts every morning, were ordered. The good effects of this treatment soon became apparent. The symptomatic fever was wholly subdued, and the morbid changes in the cornea arrested. At this period mercury was again had recourse to. The amendment was now progressive. The restoration of the cornea to transparency, and the recovery of vision, were rapid and complete beyond all expectation. Red precipitate ointment was interposed to assist the restoration of the cornea.

Case II.—Mary L. aged 13, of a scrofulous habit, has been ill with ophthalmia in the left eye for ten days. The following symptoms were observed. The vessels in the sclerotica minutely injected; a vascular zone beginning to form at the verge of the cornea; pupil extremely contracted; aqueous humour muddy; cornea slightly opaque; vision very dim; aversion to light at present inconsiderable; no pain in eye or head.

The following remedies were actively employed: V. S. ad $\frac{3}{4}$ xij.; nauseating doses of tartrate of antimony; saline purgatives; blister behind the ear, kept running; the steam of warm water containing laudanum to the eye; belladonna to the eyelid; leeches were also repeatedly applied to the temple; and finally, calomel and opium twice daily. Notwithstanding these means, the inflammation continued its progress; and in the course of four or five weeks the cornea was rendered completely opaque. The pupil cannot be seen; vision entirely gone; intolerance of light excessive; symptomatic fever very high. The right eye has become affected. Mercurial inunction was superadded to the calomel and opium, and in eight days salivation was established. This, so anxiously looked for, entirely disappointed me. At this period the last affected eye presented a most alarming appearance. The cornea at its junction with the sclerotica was of an intense red colour. This appearance extended some lines over the corneal margin. The cornea was very much thickened, and red vessels were seen ramifying over its entire surface. The iris was entirely obscured; aversion to light urgent; vision in both eyes was quite extinguished. Sarsaparilla was substituted for the mercury. And to subdue the irritability of the system, emetics, Dover's powder, and saline laxatives, were ordered, as in the former case.

These means very soon interrupted the march of the disease. As soon as this was accomplished I again commenced a mercurial course; and although several days elapsed before the constitutional effects of the mineral appeared, its effects were most decisive.

The improvement continued progressive. Red precipitate ointment and vin. opii were applied to the eyes; and in a short time the cornea was restored to transparency, and a perfect state of vision obtained.

When the acute symptoms began to yield, the conjunctiva, which had all along taken so little share in the inflammation, became highly vascular. There remained a faint appearance of the vascular zone at the verge of the cornea for a considerable time, which, however, gradually disappeared.

Having met with this case not long after the first, I had still to learn the obstinacy of the disease under the ordinary treatment.

When this case presented itself, I resolved on pushing two of the most powerful remedies to their fullest extent; and what was the result? The loss of blood, by reducing the system, rendered the case almost irremediable. The too early use of mercury, on the other hand, hurried on the disease, and increased the irritation to an alarming extent.

From the Edinburgh Medical and Surgical Journal.

SUGGESTIONS FOR THE IMPROVEMENT OF THE FEMALE CATHETER.

By WILLIAM F. MONTGOMERY, A. M., M. B.,
Professor of Midwifery in the King and
Queen's College of Physicians in Ireland.

The introduction of a catheter into the female bladder, and the evacuation of the urine by its means, is confessedly a very simple operation; but as it is also an operation from its nature very revolting to female delicacy, and regarded by women in general as much more simple than it really is, they are apt to be much influenced in their feelings by the neatness and dexterity with which it is performed.

If I had no other reasons than these for the suggestions which I have to propose for the adoption of the profession, I should consider them fully sufficient to warrant me in so doing. But I am inclined to hope that some consideration of higher importance will be found connected with the alteration in the construction of this instrument, which I have to recommend from experience of its advantages.

The female catheter in general use in this country, has no other provision for preventing the instantaneous flow of the water, as soon as its point enters the bladder, than the insertion of a plug attached to the stilette. This plug is hardly ever fitted with such accuracy, even at first, as to prevent the escape of the urine from the end of the instrument before a vessel can in many instances be placed to receive it. This is a defect of construction which is made a subject of very general complaint, as causing on ordinary occasions a disagreeable inconvenience, and which, under particular circumstances, may be productive not only of very considerable annoyance, but even serious injury to the patient.

To remedy this defect in the first place, and

with other intentions to be afterwards noticed, I wish to propose, instead of the plug, the adaptation of a stopcock, by means of which we are enabled completely to prevent the escape of a drop of urine until we are prepared to receive it.

In cases of retention of urine occurring under circumstances which preclude the possibility of repeated visits from the medical attendant, the use of an instrument so constructed will enable the patient herself or an attendant to let off the contents of the bladder in the intervals of the physician's visits, the instrument having been previously secured in the urethra by passing the tapes of a bandage through the eyes or loops on its sides, and interposing a soft thin cushion, through which the catheter should pass.

I should here observe that it is not necessary that the catheter should be of metal, as the metallic bell containing the stopcock may be securely attached to a portion of the flexible or common gum elastic catheter.

Another inconvenience experienced, even in common cases, is the application of a vessel to receive the water. We cannot always prevail or insist on a patient to assume the position best suited to our purpose. Change of posture may be prevented by pain, or equally by the excessive confusion of shame with which she is overwhelmed at the idea of the operation. Then the vessels usually met with in bed-chambers are, from their height and size, particularly unfit and difficult to apply readily under the extremity of the catheter, and their apposition very apt to be attended with some degree of exposure, if the patient be not placed completely over the edge of the bed. Again, in the removal of such vessels it very constantly happens that some of the urine is spilled into the bed, or that the bed clothes fall into the vessel, in either case causing much discomfort to the patient.

In order to obviate such inconveniences, and to avoid the necessity of having to seek any receiver, I have adapted to the bell of the catheter a silver cap, to which is attached a fine bladder, such as that of a calf or sheep, prepared for the purpose by moistening it a little with oil. An instrument provided with this addition is not in the slightest degree less manageable than when used without it. The bladder lies in the palm of the hand, without in the least interfering with the management of the instrument by the fingers; or the catheter may be first introduced, and the cap afterwards slipped on, without any difficulty or delay. At the same instant the stopcock may be turned, and the urine is discharged into the attached bladder, without further disturbance of the patient; without even the apprehension of exposure on her part; and without her experiencing what, under some circumstances, might be a matter of no small importance, the feeling of cold, either from air admitted in removing the bed-clothes, to allow the introduction of a large vessel, or from the chilling contact of the vessel itself.

The circumstances of a case every day met

with in practice, will perhaps best illustrate the advantages to be derived from the use of an instrument so provided. We are called to see a patient with retention of urine, accompanying perhaps the partial paralysis, which is sometimes a consequence of severe labour, or arising from some severe accident, or the effect of fever. We find such a patient lying sunk in the middle of a large bed, totally helpless and incapable of turning on either side, or even of being turned without great disturbance or suffering. In a case such as this, with the patient so situate, if a catheter be introduced, its end is so much depressed towards the bed, that we cannot apply under it any vessel, commonly to be had in the sick room; and in order to make the attempt, the limbs of the patient must be either elevated by an attendant, or considerably separated. Here is incurred the risk of striking the vessel against the end of the catheter and causing pain, or at least irritation; or spilling the fluid, an accident which causes serious inconvenience.

These objections, I think, are completely obviated by using an instrument constructed as I have advised.

I hoped at first that this instrument might be found of service in those cases of incontinence of urine which are caused by sloughing of the neck of the bladder from pressure during labour, but the urine escapes between the catheter and the edges of the aperture; at least it did so in the only case in which I had an opportunity of trying it. But a case might perhaps occur in which the breach would be so small as to be sufficiently commanded by the instrument, though in such cases I fear little can be expected from it. In some other cases of incontinence, however, its use will, I hope, be found peculiarly suitable. Other applications of the contrivance may occur to other practitioners; and an intelligent friend suggests that it might be conveniently used in the operation of injecting the bladder.

It will be found convenient to have the cap perforated with six or eight holes round the end which projects into the attached bladder; that, in case of the bladder lying on and stopping the large aperture, the water may escape by the lateral openings.

From the Medico-Chirurgical Review.

CASES ILLUSTRATIVE OF SOME OF THE APPEARANCES OBSERVABLE AFTER DEATH, when Dropsical Effusion has been connected with Disease of the Liver. By RICHARD BRIGHT, M.D.

[Reports, &c. from Guy's Hospital.]

"Quum vero a splene aut HEPATE in hydro-pem transitus fit, effugiunt non valde."—HIPPOCRATES, *de Affectionibus*, lib. xiv. cap. xxiii.

"Neque ignoro, Erasistrato displicuisse hanc curandi viam: morbum enim hunc joci-noris putavit: ita illum esse sanandum."—CELSUS, lib. iii. 21.

"Corrupti jecoris vitio vel splenis acerbus crescit hydrops."—SERENUS SAMONICUS.

The three quotations which we have introduced from three of the most ancient works on medicine, will show at what an early period dropsy was traced to disease of the liver. Erasistratus, like some hobby-horse riders of modern times, wrote a book to prove that dropsy was *always* dependent on hepatic disease; but the remark of Celsus on this doctrine is equally terse and true. "Sed primum, non hujus visceris unius hoc vitium (hydrops) est: nam et liene affecto, et in totius corporis malo habitu fit." Loco citato. We shall not, however, wade through the innumerable speculations which have been entertained as to the nature, causes, or treatment of the different species of dropsy. We shall only remark one or two curious facts; first, that the father of physic recognized certain cases of anasarca which occurred suddenly in robust constitutions, and required blood-letting.—*De Victu Acutorum*, lxii. How far Hippocrates anticipated modern pathologists on this point, it is not necessary to inquire. The second remark is, that Areteus appears first to have noticed hydatid—or rather ovarian dropsy, in which paracentesis was not effectual. This sagacious disciple of Esculapius made a confession, which, though unpalatable to the "march of intellect" in the present day, contains, nevertheless, some truth. He tells us that few have been cured of dropsy—and that those who have been so fortunate as to escape, may thank the gods, rather than the doctors, for their recovery!

But we must come at once to the work before us. In our last number, we gave an analysis of Dr. Bright's first division, in which an attempt was made to prove that many cases of dropsy depended on certain organic changes in the kidneys themselves. The next division relates to the etiology of dropsy, as connected with hepatic disease. Dr. B. sets out by expressing a conviction, that many cases of this effusion are owing to renal, which are set down to the account of hepatic affection—but, at the same time, he entertains no doubt that, "in many other cases, the liver is the real cause of the dropsical effusion, frequently showing most extensive disease, when the kidneys are quite healthy." In fact, it did not escape Dr. Bright's notice—and it cannot escape the notice of his readers, that, in almost every case which he has detailed of *renal* disease, in connexion with dropsy, there was also more or less of disease in the liver. The dissections of Morgagni, Bonetus, and Lieutaud, corroborate the facts disclosed by Dr. Bright, and all these circumstances, taken in connexion, tend to throw a considerable shade of doubt on Dr. Bright's doctrine of the renal origin of dropsy. The same doubt, however, does not apply to the present section of the work under review.

In the preceding section, Dr. B. had often expressed himself thus:—"The liver showed a tendency to granulation"—thus intimating

a doubt as to the existence of actual change of structure. Dr. B. explains himself in the following terms.

"The fact is, that the liver in these cases has usually preserved its natural figure; the acute margin has been perfect, and the general size has not been augmented; the peritoneum has been quite transparent, and attached only in the ordinary degree to the viscus; the texture of the liver has neither been unnaturally firm nor morbidly flaccid; but, on examining the surface, it has been evident that the colour was less uniform than in perfect health: the whole was marbled, consisting of very small light spots in a darker ground; but on making a section perpendicular to the surface, though the same general variety of colour has been observed, yet in some parts of the section it has been doubtful whether the darker or the lighter part should be considered as the ground-work: in general, however, by attentive observation, it will be found that, in the centre of the lighter spots, small depressions or openings are visible, and that the darker parts appear to be the connecting medium of the lighter parts, which seem to be the acini of the glandular structure. Although in most cases these appearances scarcely attract attention, yet in other cases they become more obvious, either the white portions becoming larger in proportion, or the whole viscus appearing to have lost a little of its natural pliability, to have become hard, and to break down with a slightly granulated fracture."

In all these cases, Dr. B. observes, "the secretion of the bile is tolerably natural, the gall-bladder being well supplied with bile, of a sufficiently dark yellow colour." We have reason to believe, that much error has arisen respecting the healthy secretion of the liver, because the colour of the bile has appeared natural in the gall-bladder and in the secretions. It should be recollected that it is in the gall-bladder the bile assumes that colour which tinges the faces—and that only a certain portion of the bile that passes into the intestines has ever been in the gall-bladder. Now the biliary secretion may be much changed, and yet that portion of it which regurgitates into the above mentioned reservoir may there assume the *usual colour*, while greatly deficient in its other qualities. We must not, therefore, be guided entirely by the colour of the motions, but by the smell, and other properties. How often do we find the secretions unbearably fetid, while their colour is brown, or even yellow? The states of the digestion, the complexion, nay, the urine itself, are all influenced by the state of the biliary organ.

Besides the appearances above described by Dr. Bright, in the livers of dropsical subjects, he has occasionally seen the organ deviating in its consistence from the natural state, "being either too firm or too flaccid," though such changes are doubtless seen where no dropsical effusions obtain. From the prominent place which the renal disease appeared to hold in these cases, our author was induced

to consider the hepatic derangement as secondary or subordinate, "though not impossibly the state of both these organs depends on the same general constitutional affection; and I have sometimes even thought that the tendency to granulation, where it existed, maintained a certain relation, in its progress, to the disease in the kidney." Moreover, the author justly remarks, that there are hepatic derangements, unaccompanied by obvious disease in other organs, which may probably be, with propriety, considered as laying the foundation of dropsical effusions. To a detail of cases of this kind Dr. Bright next proceeds, and we shall follow him.

Case 1. W. Taylor, aged 66, by profession an architectural drawer, but now a pauper, was admitted on the 4th January, 1826. He confessed that he had lived hard, and it was evident that reverses of fortune and disappointed hopes, might have injured his *morale*. For the four preceding months, his appetite had failed, and still more recently his legs began to swell; his abdomen became tumid, and his flesh wasted. His countenance was sallow, conjunctivæ slightly yellow, bowels relaxed, pulse weak, abdomen tender on pressure, especially in the region of the liver—and altogether he had the appearance of being completely broken down. The motions did not seem deficient in bile, nor unnatural in colour; but the urine was loaded with a pink sediment, and was not coagulable by heat. We need not give the details of 16 days' unsuccessful practice. On the 20th January he sat up, and seemed more revived. In the evening he was seized with dyspnoea, and died in the night.

Dissection. No effusion into the cavities of the pleura—very little of the lungs was crepitous; yet they were not hepatized nor tuberculated. They were congested, and loaded with serum—in short, they were œdematous. Heart sound—abdomen contained seven or eight pounds of clear straw-coloured serum—body and limbs œdematous. The intestines unaffected. Liver contracted, and of a morbid structure throughout, apparently from depositions of minute portions of yellow matter. The surface presented a general rough granular feel, the colour being a liver red and yellowish gray. The same structure pervaded the whole of the interior. The liver was, upon the whole, smaller than natural, and broke down easily with a brittle crisp fracture, uneven and granular. The gall-bladder was opaque and thick, containing the usual quantity of bile. The orifice of the ductus communis was contracted into a nipple-like projection, with an orifice not larger than to admit a pin. The gall-bladder contained a deep-coloured viscid bile, and, when emptied, presented, on its internal surface, a number of minute yellow bodies, rather larger than millet-seed, and soft. The pancreas was soft; spleen very small; the kidneys smaller than natural, but perfectly healthy.

In the above case, from the appearances on dissection, Dr. Bright was led to suspect, that

part of the structural change in the liver might depend on some deposit from the bile similar to that which obtained on the inner surface of the gall-bladder. Dr. Bostock was, therefore, applied to, for the purpose of chemically examining into this point. Dr. B.'s chemical processes are given, in a letter from that gentleman; but we shall content ourselves with the results.

"From the above observations I think we are warranted in concluding, that the liver which you sent me for examination contained a quantity of a substance nearly resembling cholesterine, the body which forms the basis of the biliary calculi. I do not venture to determine concerning the nature of the connexion which subsisted between this substance and the liver, but I should conjecture that it had been secreted by the arteries of this organ, and deposited in its cellular texture."

Case 2. J. Macdonald, a youth of 15, was admitted on the 21st of June, 1826. He was of weakly constitution, but said he had enjoyed good health till within two months. At that time his legs began to swell, and latterly his abdomen. It was now considerably enlarged, and a tumour could be distinctly felt in the region of the liver. The legs were slightly œdematous—emaciation general—urine scanty, and *not* coagulable. Diuretics and mercurials were given, and some slight improvement ensued; but, about the beginning of September, the boy became evidently worse, and on the 27th of that month he was tapped, when three gallons of straw-coloured serum were drawn off. On the first of October, the tumour of the liver is reported to be felt completely tuberculated. He died exhausted on the 16th October.

Dissection. There were slight marks of peritoneal inflammation, and some flakes of coagulable lymph in the abdominal effusion. The liver was externally tuberculous, of a light yellow colour, with deep fissures on its surface, apparently arising from partial contractions in the substance of the organ, or its adventitious investing membrane. The liver, which was about one third larger than natural, was also increased in density and specific gravity, cutting with considerably more resistance than boiled udder, to which it bore some resemblance. Its whole structure was composed of bright yellow granules, distributed in a transparent pinkish ground, the two parts bearing nearly an equal proportion. There was no appearance of tubercular structure in the organ. The gall-bladder was contracted, containing a small quantity of dirty-looking bile—kidneys rather pale, with irregular vascularity—lungs and heart quite healthy.

Case 3. Thomas Holbeach, aged 60, was admitted on the 12th October, 1825, in a lamentable state of dropsy. His whole body was unwieldy—his legs and thighs greatly swollen, with ill-conditioned ulcers on his shins. He lies propped up in bed, continually moaning. Urine is scanty and rather high-coloured—motions frequently loose, but not very deficient in bile—tongue dry and

brown. He was ordered squills, blue-pill, and opium, with some diuretics; but although he sometimes showed symptoms of amelioration, he sunk exhausted on the 23d November, about five weeks after he came to the hospital.

Dissection. On opening the abdomen a singular appearance presented itself to view, when the water was drawn off. All the viscera stood rigidly raised like rock-work. The liver formed two whitish flesh-coloured masses, the edges thickened and rounded, and the whole surface somewhat tuberculous. Below the liver, to the left, was an irregular mass, purplish in colour. It was found to be a mass of omentum and colon matted together by an adventitious membrane, which appeared to cover the whole. Below this were seen four or five convolutions of intestine perfectly erect and stiff, of a purplish green or livid colour, covered by the same adventitious membrane. These convolutions felt thick, hard, and elastic. The substance of the liver was found hardened throughout, the structure nearly resembling scirrhus, with bands of thickened cellular membrane, like ligamentous membrane pervading every part—in some places forming one third of the whole structure. There were no tubercles in the interior of the organ, which felt nearly as hard as cartilage. There were old adhesions between the liver and diaphragm. The gall-bladder was contracted, and covered by false membrane, and contained bright yellow bile, the ducts being pervious. The coats of the intestines were, in some places, the sixth of an inch in thickness. The kidneys were healthy—the lungs, in some places, œdematous and flabby, but, on the whole, not unhealthy. There was some effusion into the cavities of the thorax and pericardium.

Having thus given three out of the seven cases detailed by our author, in illustration of his subject, we do not deem it necessary to notice any more.

Dr. Bright observes that the foregoing cases present at least three distinct morbid conditions of the liver, all terminating in dropsical effusion into the abdomen. Thus, in one case, (W. Taylor) a distinct morbid deposit, or a conversion of matter had taken place around or in the secreting portion of the organ, which, without interfering with the natural consistence of the liver, rendered its surface rough, and its whole texture deranged and granular.

In another case, (No. 2, Macdonald) both the secreting part and the connecting cellular tissue of the liver had suffered a change of structure nearly in an equal degree, so that the whole viscus was brought to an unusual state of firmness. The acini were enlarged, and the parenchymatous substance was thickened, and brought to a state of semi-cartilaginous hardness, without being drawn into bands.

In a third case, (Holbeach) the diseased state of the cellular membrane seemed to have advanced much further, so that it had formed bands in various directions, not unlike a scir-

rhous degeneration either in the appearance or the consistency which it assumed. "Yet the secretion in the organ had not been entirely obstructed."

In one case there were no semi-cartilaginous bands of hardened cellular tissue, but the whole organ was changed into globular concretions, harder and more tough than in the natural condition—easily picked out of the cavities in which they were imbedded, and sliding pretty readily over each other, so as to render the whole tough and pliable. In some, there will be found cysts or tubercles—in others a series of abscesses—in short, it would be endless, and moreover useless to enumerate the almost infinite variety of changes which may be seen in diseased livers.

We now proceed to notice Dr. Bright's mode of explaining the way in which the hepatic disease produces the dropsical effusion.

"It appears," says he, "that all those (organic changes) just now described produce very general obstruction to the circulation through the branches of the vena portæ, and become, in this way, the immediate cause of dropsical effusion, independently of any morbid condition which may result to the blood by its not having given off those substances from which it is purified, while the process of secreting bile is carried on in its full extent. It is these general changes in the structure of the liver which give rise to dropsy, more frequently than any of the circumscribed changes,—as tubercles of various kinds, and hydatids occurring imbedded in the substance; for the influence of these, as long as from their situation they make no immediate pressure on the large vessels, is often very small in favouring serous effusion, however much they may wear out the constitution by the irritation they produce."

For a long time we were in the habit of taking this mechanical view of the production of serous effusions in diseases of the liver; but we have given it up, and believe that it is quite erroneous. That mechanical pressure on veins will produce serous effusions or infiltrations, we admit; but, where is the proof that there is any actual obstruction to the mere flow of blood through the vessels of the liver, in diseased conditions of that organ? In many of the dropsical cases, the organ was not enlarged, but rather diminished, and, consequently, could not press on the contiguous vessels returning blood to the heart—in others, the liver was only triflingly enlarged. Yet we frequently see both the liver and spleen enlarged to an amazing size, without any symptom of dropsy. How many tumours do we find developed in the abdomen, and arrive at an immense growth, which must press upon all the neighbouring vessels, yet without dropsy. These facts did not escape the penetration of the illustrious Bichat, who resolutely denied that dropsical effusions in the abdomen and body generally are to be looked upon as resulting from mechanical obstruction to the passage of blood through the liver—an obstruction which, in fact, has never been shown

to exist. But it will be said that Lower tied the cava inferior of a dog, and produced dropsy of the abdomen. This was effecting mechanical obstruction with a vengeance! When Dr. Bright, or any who support his doctrine, show us this obstruction, by injections of the organ, we shall then admit it as the cause of dropsy, but not till then.

It is far more reasonable to conclude that either the causes which produce the liver-disease effect also the dropsical disposition—or, that the altered, and consequently morbid condition of the biliary secretion leads to the serous effusions, by disordering the functions of various organs in the animal economy, including the kidneys, thus vitiating the blood and all other fluids in the body. We have already commented on the fallacy of concluding that bile was healthy if it had the usual colour. The experiments of Dr. Bostock, appended to this section of our author's work, prove that the qualities of the bile were unequivocally deteriorated in the various specimens examined. Thus, in one specimen, upon minute inspection, some portions exhibited a yellow tinge, while others were of a light flesh-colour.

"The flesh-coloured part seemed to consist of a dense substance of a uniform texture, while the yellow part appeared to be composed of a number of irregular spots, which gave the peculiar colour to this part imbedded in the dense substance."

In another specimen, Dr. Bostock observes, "the bile was considerably lighter coloured than natural, less viscid, and had a very nauseous odour. It became rapidly putrid, and was then extremely fetid." By chemical examination, he found it to contain a substance that might be considered intermediate between albumen and mucus, "while the resinous or proper biliary matter was much more than ordinary."

In a third specimen, besides other changes, Dr. B. observed "a number of black particles diffused through it, which very slowly subsided." In a fourth specimen, the bile was "unusually thick and tenacious, and of nearly a black colour." It was found to contain a large quantity of a mixture of albumen and mucus. In a fifth specimen, the bile contained a considerable number of biliary calculi, from the size of a pea to that of a grain of sand. In a sixth specimen, the fluid would not have been "recognized as bile," had not Dr. Bright informed Dr. Bostock that it was taken from the gall-bladder. Its consistence was like that of serum, but more tenacious—its odour offensive—its colour a bright light orange. It was neither acid nor alkaline.

Now, in many of the cases from which these specimens of vitiated bile were taken, the motions had a nearly natural colour, and hence it was concluded that there could be nothing wrong with the function of the liver, whatever might be the degree of organic change. That this is a great error, we may infer from analogy as well as observation. Do the lungs perform their proper function when disorganized? Do

the kidneys secrete healthy urine when their structure is altered? Certainly not. And why should we expect healthy bile (whatever may be the colour) from an unsound liver?

In carefully examining the dissections given by our author, we scarcely find a single instance in which the peritoneal surface was not in a diseased state—either covered with false membranes, or showing other unequivocal signs of previous inflammation. This fact at once does away with the theory that the dropsical effusion is the result of mere mechanical obstruction to the flow of the blood through the liver. The fact is, that the pleura, as well as the peritoneum, takes on a morbid condition in these hepatic diseases, and hydrothorax, ascites, and anasarca, are generally combined.

A perusal of the cases brought forward by Dr. Bright, and a careful observation of facts in actual practice, would lead us to conclude that the structure or function of the liver was disordered, when dropsical effusions and waste of flesh appear in an individual not labouring under any organic disease of the heart or lungs, and who has not been recently subjected to those causes which induce an inflammatory dropsy. To this conclusion we would be induced to come, even if the stools were yellow—seeing, as we have done, that in one of the worst specimens of bile examined by Dr. Bostock, the colour of that fluid was a "bright light orange." Now, in such a case, if the alvine secretions were found of the same colour as the hepatic, the practitioner would exclaim—"Oh, here is bile as healthy as that of an infant!" Yet Dr. Bostock says, in his letter, that he should not have recognised it chemically as bile, had not Dr. Bright assured him that he took it from a human gall-bladder! Such are the fallacies to which the science of medicine is subject. Every path we tread—every step we take—every indication we act upon, is pregnant with such errors; and the whole practice of medicine, in fact, requires cleansing, as much as did the stable of Augeus.

Tales of error have been hummed into the infant's ear, while rocked in his cradle, or fondled at the breast—they have been engraven on his sensorium and some other parts, at school, by dint of the birch—they have luxuriated into gorgeous forms of classic and philosophic imagery in cloisters and colleges—they have been delivered, ore rotundo, in the assumed garbs of solemn truths and scientific dogmas, in the dissecting room, the class room, and the clinical ward—they have rolled and do roll, in volumes from the press, with all the impetus and velocity which high-pressure engines and the power of steam can confer—the false notes have been circulated so freely as sterling ore, that nobody thought of examining the water-mark of truth—in short, these tales of error have been more greedily perused than the book of nature, as fostering at once the indolence of the mind, and the ease of the body; and, in this way, the field of medicine has become choaked up with weeds that will require many centuries to root out! We must return again to the paths of

Hippocrates and Sydenham--and close observation at the bed-side of sickness must supersede the theories of the closet, and the dreams of the chemical and the mechanical philosophers.

From the Edinburgh Medical and Surgical Journal.

ON CUTANEOUS ABSORPTION.

The subject of cutaneous absorption, and of absorption generally, has been lately investigated by M. Collard de Martigny. He has succeeded in establishing completely the reality of absorption through the skin, and has thrown considerable doubts over the doctrine advanced a few years ago by Magendie and Fodera, and now so generally adopted, that substances are absorbed throughout the animal system by a process of simple mechanical imbibition. As his experiments on these two topics appear to us important and conclusive, we shall give a short account of them. It is well known that Seguin, Magendie and other anterior physiologists have maintained, that the skin, when not deprived of its cuticle, is incapable of absorbing any substances which do not irritate the cuticle, or alter it in one way or another. This, however, does not appear to be the general opinion; yet we are not aware that any very conclusive arguments have hitherto been brought forward to prove the existence of simple absorption through the cuticle. The vagueness of the foundation on which the general opinion rested led M. de Martigny to endeavour to establish it upon more solid facts; and he has made a great variety of experiments, which seem to put its accuracy beyond all question.

He first placed upon the palm of the hand an inverted glass funnel filled with water, and remarked an obvious diminution of the water in the tube of the funnel after an hour, or even less. He then varied this experiment, using precautions to preserve the pressure of the hand on the funnel uniform, as well as to prevent the evaporation of the water, and he still observed a sensible diminution. The following very simple experiment gave equally pointed results: In two small vessels of the same form and size he placed equal quantities of water, one being placed at his side exposed on the surface to the air, while into the other he put his two hands for half an hour. He then removed them and dried them in a handkerchief, the weight of which was taken both before and after this operation. The water of the two vessels was likewise weighed, and the proper correction being made for what was removed by the handkerchief from the hands, he found that 178 grains had been lost by absorption. Similar experiments were made with milk and with beef-tea, and the results were analogous. It appeared to him that the beef-tea was absorbed more rapidly, and milk more slowly than water. On subsequently resuming the investigation he employed a still simpler method. He placed a few drops merely of the fluid to be examined on the inside of the arm, covering them with a small watch-glass, and

allowing the experiment to go on for eight or twelve hours and upwards. In this way he remarked that water and brandy were absorbed altogether, and wine, beef-tea, and milk almost entirely. On next extending his researches to oleaginous substances, he arrived at the singular conclusion, that cutaneous absorption is not exercised upon them at all. Our readers will perhaps remember that Bichat, Magendie, Legallois, and others, have found that oleaginous fluids, directly introduced into the blood, obstruct its passage through the capillaries, and excite inflammation there. They likewise appear to undergo a species of digestion before they enter the lacteals, when they are swallowed as food; and further, when placed in contact with the serous membranes, they are absorbed very slowly, and not till they undergo a species of saponification. In a state of purity, therefore, they cannot enter the blood without causing serious injury. In the alimentary canal this is prevented by their digestion; in the serous cavities by their saponification through means of the serum. On the skin, where there is neither gastric juice nor a serous fluid, it is prevented by their being incapable of passing the cuticle. M. de Martigny found that they did not lose an atom of weight, after being eleven hours in contact with it.

The author concludes this department of his subject by endeavouring to determine the circumstances which regulate absorption through the skin. He has discovered that it is most rapid on those parts of the body where exhalation is greatest. He has farther remarked, that, while exhalation is going on, there is no absorption; that absorption is diminished during exercise or by an increase of temperature; and that it is increased by a depression of temperature or by the evacuation of blood. In short, cutaneous absorption is influenced by every particular in the inverse ratio of cutaneous exhalation.

The other question which the author has endeavoured to investigate relates to the mechanism by which absorption is effected. The experimental inquiries of Magendie and Fodera are conceived by many physiologists to be conclusive of the fact, that absorption is carried on by a process of simple imbibition; in other words, that in reaching the absorbent vessels substances percolate the living exactly as they do the dead tissues. It has always appeared to us, however, that the experiments of Magendie were liable to important objections, and that even the more conclusive experiments of Fodera, while they prove that mechanical imbibition or filtration does sometimes occur in the living body as through dead membranes, do by no means also prove that this phenomena is not materially under the influence of the vital principle. The experimental reasoning of these two physiologists has been very ably criticised by M. de Martigny, and its fallacies exposed. To the experiment of Magendie, which consists in insulating a vein and applying an active poison to its surface, he very properly objects that the poison

may have entered the blood not by filtration but through the *vasa vasorum*. He admits, however, the conclusiveness of the experiment of Fodera, in which he found that substances injected into the peritoneum might be soon afterwards discovered in the chest; but he insists that the existence of such an imbibition is no argument against the existence also of a special action of absorption; and such a special action has, we think, been proved by him to occur in the following phenomena. Into the stomach of animals killed the previous day he injected a weak solution of hydrocyanate of potass, and four minutes afterwards he observed that the outer surface of the stomach became of a uniform blue colour when tested with the sulphate of iron. He then repeated the same experiment on living animals with the following results. Twenty minutes after the injection of the hydrocyanate, when none of it had reached the outer surface of the stomach, he poured several drops of the sulphate of iron over it. It was not till five minutes afterwards that he remarked here and there a faint blue tint not uniformly diffused as in the former instance, but arranged in a fine network; gradually the tint deepened and extended itself, its reticulated structure became less distinct, and at length a uniform blue stain was formed, as in the dead animal. It is not easy to see to what the slow progress of the fluid through the living stomach is to be attributed, unless to the permeability of the tissues being modified by their vitality. He has also endeavoured to establish the doctrine of special absorption by the following experiment, in which an attempt is made to show that absorption is under the influence of the nervous principle. Having divided all the nerves which supply the hind leg of the rabbit, he tightened a ligature round the whole thigh, exclusive of the vein and artery, and introduced a solution of the extract of *nux vomica* into the lower part of the leg. He then found that the poison operated more slowly than when the nerves were left entire. In conclusion, therefore, from these and other familiar facts, the author argues that the new doctrine of absorption must be modified by a partial return to the old; and he adopts the opinion, that this function is carried on by a special and elective action of the capillaries.—*Arch. Gen. de Med.*

From the *Lancet*.

SUCCESSFUL CASE OF TRANSFUSION.

By J. HOWELL, Esq. Bridge street, Southwark.

There has rarely been a more important operation proposed to the profession than that of transfusion, and if on further trials it proves deserving the character it would seem entitled to, it will reflect the highest honour upon Dr. Blundell, and be the means, in the hands of enterprising men, of saving the lives of many of the most interesting part of the creation, who would fall victims to uterine hæmorrhage.

A poor woman, the mother of twelve chil-

dren, 40 years of age, had a second time engaged me to attend her in her accouchment, which she expected to take place at the beginning of the ensuing month of March. On the 31st ultimo, I was hastily summoned to her, and found, that about half an hour before, without experiencing any pain, whilst quietly occupied with her needle, she had been attacked with one of the most frightful hæmorrhages I had ever seen. She was then flooding most profusely, and had lost, before my arrival, a small chamber-vesselful of blood. I immediately laid her on the bed, and, on examination, found the os uteri sufficiently dilated to admit my finger, and quite free from any connexion with the placenta. I directly ruptured the membranes, the liquor amnii escaped, the uterus contracted, and the hæmorrhage instantly ceased. The pulse was feeble, and she complained of being very faint, a state which I did not think it prudent, of course, to interfere with. I applied cold water to the uterine region, lessened the temperature of the apartment, and adopted all those precautions the serious situation of my patient called for. I remained with her some time, but as the flooding had ceased entirely, and she was free from pain, I left her, charging her friends to keep her cool and quiet, and should any pain come on, or the slightest hæmorrhage, to send for me directly. This was about half past three o'clock, P. M., and about six I was sent for, in consequence of labour pains coming on. I found she had but very little pain, and the flooding had returned in a very slight degree, but, before my arrival, had entirely stopped. The os uteri remained rigid and undilated. She was very faint, and the extremities cold; indeed the aspect of the patient was altogether alarming. In this stage of prostration of the vital powers, there was not absolute syncope, and the stomach remained undisturbed. As there was no contra-indication, I did not hesitate to administer brandy to the amount of five or six ounces; but although it rallied her for a few minutes whenever she took it, she sunk at last into such a state of collapse, that I apprehended a fatal termination, unless something more were speedily done for her. With these feelings, I requested the advice and assistance of my friend Mr. Ravis, of Union street; who, after observing the inefficacy of stimulants, and finding the woman cold, excessively cold, with an imperceptible pulse, and supervening insensibility, agreed with me, that nothing but transfusion held out a shadow of hope. Delivery had nothing to do with it: there were no pains; there had been no hæmorrhage since my second visit; therefore delivery could have availed nothing, but would necessarily have induced some loss of blood, and thus, probably, have placed the woman beyond hope. Seeing, then, that nothing but the operation allowed any chance, and knowing that our neighbour, Mr. Doubleday, had acquired some tact in the performance of it, we lost no time in soliciting his assistance, which was granted us with that readiness creditable to Mr. Dou-

bleday, as a man of humanity, and one interested in the improvement of our profession. The woman was now in the most exhausted state, and could be prevailed upon to take only very small quantities of brandy, asserting, when able to articulate, that she was dying. No pulse to be felt; the respiration scarcely to be recognised; the whole body as cold as ice; the very breath cold, and with that high degree of restlessness so characteristic of the last stage of these lamentable cases, it was very evident the patient could not long survive.

A good supply of blood being obtained from the husband, a hearty coal-heaver, the operation was commenced twenty minutes before eight o'clock, and occupied fifty minutes, during which time twelve ounces of blood were cautiously injected. When the first five ounces had been thrown up, the pulse was perceptible at the wrist, and the patient, with increased power, declared she was better, and should do very well. At the conclusion of the operation, the woman was warmer, and continued getting so, taking, from time to time, some brandy and water, and as much warm milk as we could get her to swallow; still the pulse remained indistinct and fluctuating. At about nine o'clock the pains returned, and the os uteri quickly dilating, in about an hour after the transfusion, the patient was delivered of a dead child, the placenta almost immediately following. By carefully kept-up pressure above the pubes, the uterus was made to contract firmly, and very little blood followed the after-birth; yet there was enough to produce a sensible effect on our patient, again inducing the coldness and faintness slightly. Fortunately, however, this was removed by some brandy and water, with a full dose of laudanum, so that an hour after delivery, the woman was tolerably easy, much warmer, and inclined to sleep. We left her with a feeble pulse, but, in other respects, comparatively comfortable; and, with the assistance of another opiate, she passed a tranquil night. It may be mentioned, as showing the extreme condition of the patient before and during the operation, that she was quite insensible to any pain from it. It should be observed, too, that after the first injection, a disposition to nausea, unattended, however, with vomiting, was produced.

Did the transfusion occasion the recurrence of the pain?

It is only necessary to add that the patient has gone on very well, no untoward symptom having appeared; and she is now in such a satisfactory state, as to leave no doubt of her speedy recovery.

From the *Lancet*.

SINGULAR CASE IN MIDWIFERY. By
Dr. HARE, Great Baddow, Essex.

I beg to transmit the following midwifery case for insertion in your widely-extended publication, as the most effectual means of rescuing it from oblivion, which I feel it does

not merit, being the only one of the kind upon record.

When I resided at Southminster, Sarah Maynard, of the adjoining parish of Burnham, came to engage me to attend her in her approaching confinement; this was in the autumn of 1789. She was about twenty-eight years of age; a short, thin, delicate woman.

I arrived a few hours after she was taken in labour, on the 10th of November; the pains were strong and regular, with much bearing down. I had her laid on her left side, in order that I might examine and judge of her situation. I found the uterus low down in the pelvis, close to the os externum. I hoped the labour would soon be over, should the os uteri dilate; I endeavoured to find it in every direction, first backward towards the anus, then all round the pelvis, without being able to make any thing out. I could find no entrance into the vagina, nor could I introduce my finger horizontally between the perinæum and uterus, which I had never failed to do in all other cases, however low the presenting body might be. I felt convinced, during the last examination, that the uterus adhered all round the pelvis, and that the vagina was totally obliterated. It was about eight o'clock on Wednesday evening when I came to her, and by the time I had satisfied myself of these extraordinary particulars, the night was pretty far spent. I proposed to lie down for an hour or two, more with a view of reflecting upon the case without interruption, than with any intention of going to sleep. Her pains coming on stronger, attended with severe sickness, I was soon called up again. I amused the woman as long as I could, by wishing her to walk about a little, and when her patience was pretty well exhausted, I made another examination, with no better success. I thought I discovered the os tincæ, but it turned out to be only a furrow, or plica of the uterus. The action of the pains upon the uterus was intercepted and lost by its adhesion round the pelvis, for the presenting body never varied its position, which I had every reason to suppose was the head of the child, so far as I could judge by the feel through the uterus, not being able to find any entrance. I represented to the woman's friends the extreme danger and singularity of the case; that, for the satisfaction of all parties, and to avoid censure, I wished for a consultation. Dr. Paxton, of Maldon, and my brother, were fixed on. We met on Thursday evening. I represented the state of the case to them, and they found every thing as already mentioned. Her pains were still strong and regular; constant sickness and vomiting; complained much of violent pain in the abdomen, which made me fearful of rupture of the uterus, as the resistance at the outlet was so great.

Upon mature deliberation, we concluded little could be attempted with any prospect of success. Nothing suggested itself except the Cæsarian operation, at any time formidable, but under present circumstances not jus-

tifiable, on account of the appearance of uterine and peritoneal inflammation coming on.

She had opiates administered from time to time to alleviate her sufferings. We questioned the husband whether he had found any impediment or obstruction in his intercourse with his wife; he declared, no longer than about two or three months, when she complained of pain. When she spoke to me to attend her, she informed me she had lain in of her first child about eight years before; that she had a very bad time, and was attended by two surgeons.

Being desirous of gaining every information that could throw any light on this mysterious case, I wrote a note to Mr. Passel, one of the surgeons whose patient she had been, (the other gentleman being dead,) requesting him to inform me of all or any of the particulars of her former labour that he could recollect, as her present situation was a most extraordinary one.

He politely returned me an immediate answer.—“That from his memorandum of the case the presentation was a natural one; but the death of the child not being ascertained at first, and the pains being very violent, with extreme rigidity of the os internum, rest was procured her by the assistance of opium twice. After the use of clysters, and in about three days, nature perfected the business without any help, he having left the house about two hours before, and the other gentleman being down stairs at breakfast.”

It will be seen in the sequel the great importance of this communication. On Friday morning we found she passed a very bad night; the pains continued without abatement; extremely anxious; up and down, not bearing to be confined to one posture; the countenance wild; pulse small, hard, and quick. During the pains there was a good deal of discharge from the uterus, of a thin, whitish fluid, which we could not account for; the abdomen distended, painful, and tender, not bearing to be touched. Saturday morning, slightly delirious; pulse weak and low; coldness of the extremities. She died about eight o'clock in the evening, after seventy-two hours of great suffering. The pains ceased some hours before her death, but restless to the last, as she was walking about the house a few minutes before her death.

We had great difficulty in procuring a post-mortem examination. Upon opening the cavity of the abdomen some fetid air escaped, and it contained a considerable quantity of thin whitish matter, exactly similar in appearance to what had been discharged from the uterus during the last stage of the labour. The intestines, viscera, peritoneum, and uterus, were covered with a thick layer of coagulable lymph; the uterus filled the whole anterior part of the abdomen and pelvis. Upon making a longitudinal incision into it, it was found to contain a fine full-grown child, the presentation natural, the fetus standing, as it were, upon its head, and nearly filling

the pelvis. There was an opening through the uterus, about four inches long and two wide, opposite the sacrum, evidently caused by its projection; its edges were worn thin, and seemed to be of some standing, produced by the friction of the uterus, as it increased in volume, against the projecting bone. The rectum was turned to one side of the sacrum. This opening, communicating with the abdomen and uterus, satisfactorily accounted for the discharge of the whitish fluid during the last day of the labour, being the pus thrown out into the abdomen from so extensive an inflammation. Upon examining the uterus in situ, we found at the os externum, a small, smooth, round opening through it, barely sufficient to admit the blunt end of a small probe, and from this foramen extended a cicatrix for about two inches, towards the right labium, and was lost in its adhesion to the side of the pelvis. The urethra was in its natural situation; no appearance of vagina; the uterus at the os externum, and adhering to the perinæum and sides of the pelvis; the labia were two inches long, and formed a kind of external cavity.

Had I not procured a history of her former labour, the case, as to how the laceration took place, would have been involved in much obscurity; it can, I feel, be satisfactorily explained, viz., three days in strong labour, extreme rigidity of the os internum, delivered suddenly without any assistance, &c. This could have arisen only from laceration of the uterus during the labour; hence also its consequent adhesions to the perinæum and sides of the pelvis, in consequence of the inflammation and coagulable lymph thrown out during so protracted a case. Whether the gentlemen were aware of what happened, or what sort of recovery she had, does not appear.

In a pathological point of view, I feel this case to be of great importance, and perhaps the most singular of the kind upon record. After so extensive an injury, becoming again impregnated, and sustaining a further extensive opening at its posterior side, from its friction against the os sacrum, all this with apparent impunity, shows the wonderful resources of nature.

It appears not the least singular part of this case, that the only remains of the os tinæ was the foramen already mentioned. Physiologists must decide how far this case is calculated to elucidate that wonderful and mysterious act of Providence—CONCEPTION. The sole mode of impregnation was by this small opening in the uterus; and the only lodgment for the semen was between the labia, the vagina being quite obliterated.

Should there appear any obscurity in the relation of this case, I shall feel happy to afford you or your correspondents any further information in my power, having made too deep an impression on my memory ever to be eradicated, witnessing so much distress and suffering without power to alleviate them.

I have, at some future period, to communi-

cate some extraordinary particulars of a case of retention of urine. Sir Astley Cooper mentions it in No. 13, page 410, of *The Lancet*; I am the person there spoken of. A cast of the case is in the Museum of St. Thomas's Hospital.

Great Baddow, Chelmsford, Essex,
January 1, 1828.

From the London Medical Repository.

FURTHER REMARKS ON THE INTERNAL EXHIBITION OF THE ACETATE OF LEAD, *chiefly with the view of determining to what extent it may be safely administered in the Cure of Diseases, especially in Uterine Hæmorrhage.* By WILLIAM LAIDLAW, Esq. Surgeon.

In the former remarks,* it was stated that the attempt made to determine this point had been interrupted, not by a necessary, but an accidental circumstance, and also, that the second experiment, instituted for the accomplishment of the same purpose as the first, had likewise been interrupted, by other means than those contemplated. It had been my own previous opinion, as well as that of others, that, although various accidental phenomena might result from the internal use of the acetate of lead, yet the chief and most unvarying effect was, the inducing of colica pictonum. With this impression, it was expected that the salt could have been pushed to the requisite extent for the production of that disease, and possession thereby obtained of a criterion by which, generally, a fair estimate might be formed of the quantity that might be employed for its salutary results, without the risk of inducing any of its deleterious effects. But this conclusion, so far as the following experiment goes, does not appear to have been well founded.

On the 10th of October, the following prescription was prepared, to be continued till colica pictonum was induced, or otherwise contra-indicated. *R. plumbi superacetatis 3j.; pulveris opii, gr. viii.; medullæ panis, 3j.; acidi aceticæ, q. s. Subige in massam, dividendam in pilulas octodecim æquales, e quibus, ter quotidie, una sumatur.*

When these pills were commenced, good health was apparently enjoyed, although, from the subsequently sudden phenomena, there is reason for suspecting that the constitution had not been entirely freed from the influence of the former experiment.

11th—2d day. Tongue white; taste in the mouth metallic; pulse 60; urine high coloured.

12th—3d day. Appetite good; metallic taste more distinct; gums tender and turgid; pulse 55; weakness felt at the knees; urine high coloured; bowels have not been moved since the 10th. *R. pulv. rhei; pulv. jalapæ aa 3ss.; acidi aceticæ q. s. Fiant pilulæ xiv. e quibus,*

omni quartâ horâ, donec alvus leviter solvatur, duæ sumantur.

13th—4th day. Eight of the laxative pills taken; bowels moderately moved; metallic taste, tenderness and turgidity of the gums less severe; urine more natural; debility diminished; pulse 56.

14th—5th day. All the symptoms diminished; functions natural.

15th—6th day. An unpleasant sensation of tightness in the breast, increased by exercise; metallic taste strong, not sweet, rather disagreeably bitter; gums turgid and tender; no nausea; constriction of the throat; pains of the stomach or abdomen; pulse 52; after moderate exercise, 85; heat, 96; after moderate exercise, 100½; urine high coloured; bowels confined.

16th—7th day. The uneasiness in the breast increased; metallic taste stronger; gums more turgid and tender; pulse 52, rather weak; urine high coloured; bowels confined. Repet. pil. plumbi superacet.

17th—8th day. The affection of the mouth increases in severity; the unpleasant sensation in the breast and debility are also increased; countenance sallow; pulse 58; respirations, in the space of a minute, twelve, and somewhat laborious; no nausea, pains of the stomach or abdomen; bowels confined. Intermit. pil. plumbi superacetatis. Repet. pil. rhei et jalapæ.

18th—9th day. Last night ptialism came on; all the other symptoms continue severe. This morning a sensation of tightness in the hands and feet, especially in the fingers and toes, is felt gradually diminishing in distinctness as it extends beyond those parts; the sensation is not that of acute pain, but is rather of numbness, increased, or becoming more perceptible by the using of the affected parts.

By the free exhibition of the sulphate of magnesia, all the described symptoms, with the exception of the affection of the mouth, gradually decreased, so that they ceased after the fifth day to be troublesome. The gums, however, were so tender and turgid as not to be entirely well for a month after the termination of the experiment. The ptialism was never profuse, but sufficiently copious, so as almost entirely to prevent sleep for three or four nights, and troublesome in some degree for several weeks.

Up to the eighth day seventy grains of the acetate of lead had been exhibited, and yet no indication of the complaint had appeared, the inducing of which was the chief object of the experiment. Other symptoms, however, had manifested themselves, and had made such an impression on the animal economy, as to render it a subject for consideration, whether or not it were prudent to proceed with the experiment farther. Accordingly, consulting with Dr. Henry Davies, it was agreed, that the severity of the symptoms rendered it advisable to put a termination to the process.

By this second experiment, the inference drawn from the first, relative to the quantity of the acetate of lead that may taken without

* Vide *Journal of Foreign Medicine* for May.

the risk of inducing colica pictonum, is confirmed, and extended from fifty to seventy grains.

As a further corroboration of this inference, I shall relate the substance of a case of severe uterine hæmorrhage that occurred under my observation: November last, Mrs. I—, aged 28, suckled her child nine months, four of the last five of which the catamenia regularly appeared, and at each successive period increased in quantity. Shortly after the last period it again returned, and continued for seven weeks before I saw her. Latterly the discharge was accompanied with severe pains in the region of the uterus, resembling those of labour, and with the expulsion of considerable *coagula*. A variety of astringent medicines internally were administered, also an injection, per vaginam, consisting of a solution of the acetate of lead in acetic acid and distilled water, the cold hip bath, and pressure applied to the region of the uterus; but no impression having been made on the complaint, the acetate of lead, in the same proportion as in the above experiment, was prescribed, with the addition of acidulated draughts. Such, however, was the severity of the malady, that it was not entirely overcome till seventy-five grains of the salts were administered.

Notwithstanding this extensive use of the remedy, neither colica pictonum, nor any of the severe symptoms that occurred in the second experiment were induced. The mildness of the operation of the remedy in this case is, perhaps, to be accounted for by the circumstance of the bowels having, without aid, continued quite regular (which is a rare occurrence) during the whole period of the salt being exhibited. For it was remarked in both the experiments that, whenever the bowels were suffered to become constipated, the effects of the salt became more marked, but being moved by the assistance of medicine, all the severe symptoms gradually diminished. Hence the control that will always be possessed by the judicious exhibition of this remedy.

By this conclusion, however, it is not wished that it should be understood as asserted, that in every instance the salt can be pushed to this extent without inducing other unpleasant symptoms than colica pictonum, which may render it necessary to put a termination to its exhibition. On the contrary, though colica pictonum was not induced, other symptoms did put a termination to the experiment. Besides, idiosyncrasies will, in the administration of this medicine, be found to occur as in others. One patient will be found who cannot, without serious inconvenience, take even one grain of calomel, while others can take ten, not only with impunity, but with decided advantage. So it will happen with the acetate of lead. Some patients I have found with whom it could not be carried beyond twenty grains, without inducing vertigo, nausea, and in others, even vomiting, while in the experiment, in the case of Mrs. I—, seventy grains were exhibited, without either these symp-

toms, or those of colica having been induced. The quantity, therefore, judiciously given in the latter cases, would be extremely rash and ill advised in the former.

Although, then, the conclusion, that from fifty to seventy grains of the salt may be administered, without the risk of inducing colica pictonum, will be found generally to hold good, yet much will depend on the circumstances of particular cases, which can only be positively determined by careful observation.

From this experiment it does not appear that colica pictonum is so frequently or so easily produced by the use of the acetate of lead as has been supposed. Gripes, it is true, are often thus produced; but these seem to be an incidental and not a necessary consequence, arising either from a peculiarity in the habit of the patient, a deterioration of the opium, or the proportion employed being too small. In the experiment, the opium employed was British, and in the proportion of rather more than one to eight of the salt; but there was not the slightest symptom, during the whole seven days which it lasted, of gripes, nausea, or pains in the stomach.

Should gripes be induced previous to the quantity of the remedy having been administered necessary either for the checking or removal of the malady, it does not appear that that circumstance should be considered a sufficient reason for suspending the remedy, but rather that the proportion of opium should be increased to the requisite extent for the suppression of the uneasy sensations. Viewing these sensations not in all their relations, the limited survey may induce a belief that they are really the disease denominated colica pictonum. They are, however, it is presumed, materially different, arising entirely from the irritation of the salt in passing along the intestinal tube, and can therefore be removed by the diminution of that irritation. But colica pictonum seems to be produced by the constitution being fully imbued with the metal, and which unfolds its influence on the system in regular paroxysms in the intestines, which paroxysms, though they can be moderated by opium, cannot be entirely overcome otherwise than by the removal of the metal from the system by the various excretions. The difference between the gripes, which are the effect of a recent exhibition of the salt, and the paroxysms which are the result of a longer continuance and more gradual introduction of it, may, perhaps, be fitly marked, by denominating the former a local, the latter a constitutional affection.

By neither of these affections, however, was the interruption of the progress of the experiment to be attributed, but to a combination of other symptoms, especially the affection of the mouth and breast. In the first experiment, slight indications of the former of these were perceived; but from a circumstance before mentioned, it was then uncertain whether these ought to be attributed to the action of the acetate of lead or not. In this experiment, however, there is no such ambiguity. The

affection of the mouth was clearly and indisputably referrible to the action of the acetate of lead alone.

Hitherto, of metals, the preparations of mercury alone have been ranked as sialogogues. The exclusive designation applied to these preparations, after the phenomena unfolded in this experiment, cannot, it is presumed, be considered as any longer quite so appropriate. For besides the metallic taste, the tenderness and turgidity of the gums, occasioned by the acetate of lead, there was also produced increased secretion of saliva.

It may be proper to state, that none of my patients in any degree perceived such symptoms.* But it may also be remarked that none of them, with the exception of one, took the salt to the same extent as was done in the first and second experiments; and further, that in the first experiment, it was perceived in so slight a degree, as might have been overlooked by a patient who takes medicine, not with a view of remarking on every impression, but only for the mitigation or the removal of a particular complaint. Besides, it can scarcely be expected of a patient, that the medicine shall be taken with the same regularity and care as when the object is purely to ascertain its spontaneous results. These considerations will, in some degree, account for the absence of the above symptoms.

In the fifth and sixth case of *colica pictorum*, quoted by Orfila from M. Merat, the patients are stated to have had a disagreeable and bitter taste in the mouth.† Whether this taste was metallic, from the statement cannot be satisfactorily ascertained, but the supposition that it was, is not improbable. No mention, however, is made by that author, or indeed by any other that I know, of the tenderness and turgidity of the gums, and of ptyalism. This, however, I am inclined to attribute, not so much to idiosyncrasy as to the mode of introduction, and to the quantity, in a given time, received into the system. Ptyalism, it is well known, is greatly influenced by these circumstances in the exhibition of mercury; and it would appear they have a similar influence in modifying the effects of the acetate of lead; for, notwithstanding the frequency of the occurrence of the system having been brought under the influence of this salt, yet that having happened for the most part in a gradual manner, there is reason to conclude, from the silence of authors, that ptyalism rarely, if ever, occurred. But when introduced rapidly, regularly, and to the requisite quantity, as in the experiment, it appears to be one of its natural results. Does the circumstance of the acetate of lead, of mercury, and, as is reported, of arsenic in pro-

ducing ptyalism, lead to the conclusion that all metals, if they could be taken to the requisite extent, would have a similar effect?

The other principal cause which interrupted the experiment was the affection of the breast. The sensation produced by this affection was not that of acute pain, but rather resembling the uneasiness which is the effect of moderate pressure. It felt as if deep seated, and, on taking a full inspiration or exercise, gave the sensation as if the lungs were difficult to distend.

This uneasiness, it is presumed, is to be attributed to the astringent effect of the salt, producing a somewhat tightened or condensed state of the lungs. This supposition accounts for the uneasiness being increased by a full inspiration and exercise, and it is corroborated by appearances on dissection, in one of the experiments of Orfila. He states that, on dissecting a dog, immediately after death, which had been poisoned by injecting into the jugular vein ten grains of the acetate of lead dissolved in two drachms of distilled water, the lungs were found livid in patches, their texture more tight than in the natural state, and very little crepitating.

But notwithstanding these appearances, Orfila thinks it allowable to doubt whether or not the acetate of lead acts on the lungs, and proceeds to assign reasons for his scepticism. As, however, it is conceived that the doubts relative to what may be considered the usual appearances of the lungs, as resulting from the use of the salt, are not well founded, it will be proper to give the details of the experiments which have excited those doubts.

“Experiment 1st. Thirteen grains of the acetate of lead of commerce dissolved in a drachm and a half of distilled water, were injected into the jugular vein of a small and weak dog. The injection was hardly accomplished, when the animal made three or four deep inspirations, and died without the least sign of pain or convulsions. He was instantly opened. The heart was beating forcibly; the blood contained in the left ventricle was fluid, and of a vermilion red colour; that which filled the right ventricle was also fluid; the lungs, which were of a beautiful rose colour, were crepitating, and their substance did not appear to be hardened.

“Experiment 2d. Five grains of the acetate of lead dissolved in two drachms of distilled water, were injected into the jugular vein of a robust dog of middle size—he died on the fifth day. The lungs were crepitating throughout their whole extent, and did not appear to show the least trace of injury.”

In a note, which, for the sake of reference, I shall call Experiment 3d, the experimenter proceeds: “In one of my experiments on this salt, I obtained results which appear to me important to be made known. After having injected into the jugular vein of a dog of middle size, ten grains of acetate of lead dissolved in two drachms of distilled water, the animal appeared suffocated; his respiration became difficult, panting and precipitate—and he died

* Since writing the above, one case has occurred to me, in which the patient, after having taken forty grains of the acetate of lead, complained of a bitter taste in the mouth and tenderness of the gums.

† Vide Toxicology, Vol. I. p. 472.

the risk of inducing colica pictonum, is confirmed, and extended from fifty to seventy grains.

As a further corroboration of this inference, I shall relate the substance of a case of severe uterine hæmorrhage that occurred under my observation: November last, Mrs. I—, aged 28, suckled her child nine months, four of the last five of which the catamenia regularly appeared, and at each successive period increased in quantity. Shortly after the last period it again returned, and continued for seven weeks before I saw her. Latterly the discharge was accompanied with severe pains in the region of the uterus, resembling those of labour, and with the expulsion of considerable *coagula*. A variety of astringent medicines internally were administered, also an injection, per vaginam, consisting of a solution of the acetate of lead in acetic acid and distilled water, the cold hip bath, and pressure applied to the region of the uterus; but no impression having been made on the complaint, the acetate of lead, in the same proportion as in the above experiment, was prescribed, with the addition of acidulated draughts. Such, however, was the severity of the malady, that it was not entirely overcome till seventy-five grains of the salts were administered.

Notwithstanding this extensive use of the remedy, neither colica pictonum, nor any of the severe symptoms that occurred in the second experiment were induced. The mildness of the operation of the remedy in this case is, perhaps, to be accounted for by the circumstance of the bowels having, without aid, continued quite regular (which is a rare occurrence) during the whole period of the salt being exhibited. For it was remarked in both the experiments that, whenever the bowels were suffered to become constipated, the effects of the salt became more marked, but being moved by the assistance of medicine, all the severe symptoms gradually diminished. Hence the control that will always be possessed by the judicious exhibition of this remedy.

By this conclusion, however, it is not wished that it should be understood as asserted, that in every instance the salt can be pushed to this extent without inducing other unpleasant symptoms than colica pictonum, which may render it necessary to put a termination to its exhibition. On the contrary, though colica pictonum was not induced, other symptoms did put a termination to the experiment. Besides, idiosyncrasies will, in the administration of this medicine, be found to occur as in others. One patient will be found who cannot, without serious inconvenience, take even one grain of calomel, while others can take ten, not only with impunity, but with decided advantage. So it will happen with the acetate of lead. Some patients I have found with whom it could not be carried beyond twenty grains, without inducing vertigo, nausea, and in others, even vomiting, while in the experiment, in the case of Mrs. I—, seventy grains were exhibited, without either these symp-

toms, or those of colica having been induced. The quantity, therefore, judiciously given in the latter cases, would be extremely rash and ill advised in the former.

Although, then, the conclusion, that from fifty to seventy grains of the salt may be administered, without the risk of inducing colica pictonum, will be found generally to hold good, yet much will depend on the circumstances of particular cases, which can only be positively determined by careful observation.

From this experiment it does not appear that colica pictonum is so frequently or so easily produced by the use of the acetate of lead as has been supposed. Gripes, it is true, are often thus produced; but these seem to be an incidental and not a necessary consequence, arising either from a peculiarity in the habit of the patient, a deterioration of the opium, or the proportion employed being too small. In the experiment, the opium employed was British, and in the proportion of rather more than one to eight of the salt; but there was not the slightest symptom, during the whole seven days which it lasted, of gripes, nausea, or pains in the stomach.

Should gripes be induced previous to the quantity of the remedy having been administered necessary either for the checking or removal of the malady, it does not appear that that circumstance should be considered a sufficient reason for suspending the remedy, but rather that the proportion of opium should be increased to the requisite extent for the suppression of the uneasy sensations. Viewing these sensations not in all their relations, the limited survey may induce a belief that they are really the disease denominated colica pictonum. They are, however, it is presumed, materially different, arising entirely from the irritation of the salt in passing along the intestinal tube, and can therefore be removed by the diminution of that irritation. But colica pictonum seems to be produced by the constitution being fully imbued with the metal, and which unfolds its influence on the system in regular paroxysms in the intestines, which paroxysms, though they can be moderated by opium, cannot be entirely overcome otherwise than by the removal of the metal from the system by the various excretions. The difference between the gripes, which are the effect of a recent exhibition of the salt, and the paroxysms which are the result of a longer continuance and more gradual introduction of it, may, perhaps, be fitly marked, by denominating the former a local, the latter a constitutional affection.

By neither of these affections, however, was the interruption of the progress of the experiment to be attributed, but to a combination of other symptoms, especially the affection of the mouth and breast. In the first experiment, slight indications of the former of these were perceived; but from a circumstance before mentioned, it was then uncertain whether these ought to be attributed to the action of the acetate of lead or not. In this experiment, however, there is no such ambiguity. The

affection of the mouth was clearly and indisputably referrible to the action of the acetate of lead alone.

Hitherto, of metals, the preparations of mercury alone have been ranked as sialogogues. The exclusive designation applied to these preparations, after the phenomena unfolded in this experiment, cannot, it is presumed, be considered as any longer quite so appropriate. For besides the metallic taste, the tenderness and turgidity of the gums, occasioned by the acetate of lead, there was also produced increased secretion of saliva.

It may be proper to state, that none of my patients in any degree perceived such symptoms.* But it may also be remarked that none of them, with the exception of one, took the salt to the same extent as was done in the first and second experiments; and further, that in the first experiment, it was perceived in so slight a degree, as might have been overlooked by a patient who takes medicine, not with a view of remarking on every impression, but only for the mitigation or the removal of a particular complaint. Besides, it can scarcely be expected of a patient, that the medicine shall be taken with the same regularity and care as when the object is purely to ascertain its spontaneous results. These considerations will, in some degree, account for the absence of the above symptoms.

In the fifth and sixth case of colica pictorum, quoted by Orfila from M. Merat, the patients are stated to have had a disagreeable and bitter taste in the mouth.† Whether this taste was metallic, from the statement cannot be satisfactorily ascertained, but the supposition that it was, is not improbable. No mention, however, is made by that author, or indeed by any other that I know, of the tenderness and turgidity of the gums, and of ptyalism. This, however, I am inclined to attribute, not so much to idiosyncrasy as to the mode of introduction, and to the quantity, in a given time, received into the system. Ptyalism, it is well known, is greatly influenced by these circumstances in the exhibition of mercury; and it would appear they have a similar influence in modifying the effects of the acetate of lead; for, notwithstanding the frequency of the occurrence of the system having been brought under the influence of this salt, yet that having happened for the most part in a gradual manner, there is reason to conclude, from the silence of authors, that ptyalism rarely, if ever, occurred. But when introduced rapidly, regularly, and to the requisite quantity, as in the experiment, it appears to be one of its natural results. Does the circumstance of the acetate of lead, of mercury, and, as is reported, of arsenic in pro-

ducing ptyalism, lead to the conclusion that all metals, if they could be taken to the requisite extent, would have a similar effect?

The other principal cause which interrupted the experiment was the affection of the breast. The sensation produced by this affection was not that of acute pain, but rather resembling the uneasiness which is the effect of moderate pressure. It felt as if deep seated, and, on taking a full inspiration or exercise, gave the sensation as if the lungs were difficult to distend.

This uneasiness, it is presumed, is to be attributed to the astringent effect of the salt, producing a somewhat tightened or condensed state of the lungs. This supposition accounts for the uneasiness being increased by a full inspiration and exercise, and it is corroborated by appearances on dissection, in one of the experiments of Orfila. He states that, on dissecting a dog, immediately after death, which had been poisoned by injecting into the jugular vein ten grains of the acetate of lead dissolved in two drachms of distilled water, the lungs were found livid in patches, their texture more tight than in the natural state, and very little crepitating.

But notwithstanding these appearances, Orfila thinks it allowable to doubt whether or not the acetate of lead acts on the lungs, and proceeds to assign reasons for his scepticism. As, however, it is conceived that the doubts relative to what may be considered the usual appearances of the lungs, as resulting from the use of the salt, are not well founded, it will be proper to give the details of the experiments which have excited those doubts.

“Experiment 1st. Thirteen grains of the acetate of lead of commerce dissolved in a drachm and a half of distilled water, were injected into the jugular vein of a small and weak dog. The injection was hardly accomplished, when the animal made three or four deep inspirations, and died without the least sign of pain or convulsions. He was instantly opened. The heart was beating forcibly; the blood contained in the left ventricle was fluid, and of a vermillion red colour; that which filled the right ventricle was also fluid; the lungs, which were of a beautiful rose colour, were crepitating, and their substance did not appear to be hardened.

“Experiment 2d. Five grains of the acetate of lead dissolved in two drachms of distilled water, were injected into the jugular vein of a robust dog of middle size—he died on the fifth day. The lungs were crepitating throughout their whole extent, and did not appear to show the least trace of injury.”

In a note, which, for the sake of reference, I shall call Experiment 3d, the experimenter proceeds: “In one of my experiments on this salt, I obtained results which appear to me important to be made known. After having injected into the jugular vein of a dog of middle size, ten grains of acetate of lead dissolved in two drachms of distilled water, the animal appeared suffocated; his respiration became difficult, panting and precipitate—and he died

* Since writing the above, one case has occurred to me, in which the patient, after having taken forty grains of the acetate of lead, complained of a bitter taste in the mouth and tenderness of the gums.

† Vide Toxicology, Vol. I. p. 472.

thirty-five minutes after the injection, without giving the least sign of vertigo, paralysis, or convulsions. On dissecting the body, immediately after death, the lungs were found livid in patches, their texture more tight than in the natural state, and very little crepitating."

Is this isolated fact sufficient to prove that the acetate of lead acts upon the lungs? I think it is allowable to doubt it. In fact, if this compound exerted its action on these organs, should we not have found, in Experiment 1st, the blood of the ventricle blackened? and in Experiment 2d, would not the animal, after having experienced the action of the poison during four days, have exhibited some sort of injury of the lungs?*

In regard to the first experiment it may be remarked that the blood is not, nay, cannot be found blackened in the left ventricle of the heart, unless respiration is either entirely stopped, or so much impeded as to render it laborious for some time before the action of the heart ceases. The period necessary for the producing of this effect, it appears, varies in duration, not only in different animals, but even in the same animal, under different circumstances. In this experiment, the animal being weak, and the proportion of the poison great, (thirteen grains) the shock given to the nervous system was so instantaneous and powerful as to destroy life, without allowing a sufficient interval for either affecting the colour of the blood or changing the structure of the lungs.

In the second experiment it will be observed that the animal was robust and of middle size, while the quantity of poison was small (not more than one-third,) considering it in relation to the strength of the animal the subject of the first experiment. This quantity, from the mode of its administration, although sufficient to affect the nervous system, yet was not equal to the producing of a marked or visible change in the substance of the lungs. But as changes in structure vary in degree, so their not being visible is no conclusive proof that they really do not in some degree exist. The probability is, from the extensive influence of the salt over the system, and the time it existed in it, that the substance of the lungs was affected by it, although not to such a degree as to render it obvious by inspection.

In the third experiment, the animal was likewise of middle size, and the quantity of the salt employed double that of the second. The size of the animal being the same, and the quantity of the salt double, would lead, *a priori*, to the conclusion that the results would be developed, not only in a shorter time, but also be rendered more conspicuous. The animal is accordingly found to have survived only thirty-five minutes after the injection of the salt, and that its astringent effects on the substance of the lungs were eminently marked and unequivocal.

* Vide Toxicology, Vol. 1st. Exp. I. et II. p. 460. et note p. 461.

If these remarks be well founded, they will, it is presumed, account more satisfactorily for the different results in the experiments, than by attributing them merely to the effect of accident. For there can be no doubt, I think, that the salt extends its influence over the whole system, and that the lungs, in common with other parts, are brought under its dominion. This view of the subject appears well maintained by the appearances in the third experiment of Orfila, by the sensation of stricture of the breast and laborious respiration felt by myself in the second experiment, and also by the fact, that a hæmorrhage from the lungs is as successfully suppressed by the exhibition of the acetate of lead as from the stomach or uterus.

From the Medico-Chirurgical Review.

MALARIA:—*an Essay on the Production and Propagation of this Poison, and on the Nature and Localities of the Places by which it is produced: with an Enumeration of the Diseases caused by it, and of the Means of preventing or diminishing them, both at Home and in the Naval and Military Service.* By JOHN MACCULLOCH, M. D. F. R. S. &c. Physician in Ordinary to his Royal Highness Prince Leopold. Octavo, pp. 480. Longman & Co. 1827.

The subject of this essay has undergone investigation, under various names, ever since the days of Hippocrates—perhaps from a much earlier period. Marsh miasmata—paludal effluvia—morbific emanations—epidemic constitutions—vegeto-animal exhalations—malaria, and other appellations, have been conferred on an invisible, intangible, undefinable—and hitherto unascertainable *something*, which has been only appreciated by its *effects*, but which is totally unknown in its *essence*. By a few visionaries, its existence has been denied, because its substance could not be demonstrated; but all men of sober sense and unbiassed observation have acknowledged, not only the existence, but the terrific power, of this invisible agent. The medical practitioners of our fleets and armies, from the melancholy and extensive experience which they have had of its influence in the production of diseases, where its operations have been on a large scale, bear ample testimony, in their writings, to the tremendous effects of this scourge of the human race; but, we believe that no man has ventured to extend its influence to so wide a circle, or multiply its sources to such an indefinite extent, as Dr. Macculloch. For this he will be deemed a visionary by many—perhaps by most medical men, who give themselves little trouble in such an investigation. In this journal, he will find a staunch supporter, though not an idolater. We are in the habit of thinking for ourselves—and we believe we have had a field for observation somewhat wider than Dr. Macculloch has had. We shall freely criticise and freely commend—but always under the guidance of what we

conceive to be public justice and public utility.

Dr. Macculloch will secretly acknowledge that he has ardently aimed at being *impressive*, and even *eloquent*. In the former object he has often succeeded—in the latter, seldom. With learning, science, and ample command of words, our author has been singularly unhappy in the construction of his sentences—a majority of which terminate as if something were wrong or defective, obliging the reader to re-peruse them. Dr. M. will find that an elegant or eloquent sentence must never cost the reader a thought as to its meaning and import. We make this observation, because we think that the want of brevity and perspicacity will greatly circumscribe the range of utility to which this work might otherwise extend. We shall now proceed to notice the chapters in the order of their occurrence in the book before us.

CHAP. I.—*Introductory.*

Dr. M. observes, that the existence of *malaria* has long been familiar to physicians, and even the vulgar, as the cause of intermittent fevers; but this is of little use, if the one class and the other are ignorant that, to this same malaria are owing the common fevers of summer and autumn, as well as a host of other and unsuspected diseases, as dysentery, cholera, neuralgia in various forms, nervous and dyspeptic affections—and, finally, what may be termed bad health. Dr. M. concludes, that one-half, at least, of human mortality is owing to this cause. It does not, however, bear equally hard on all countries; nor at all times in the same country. In England, for instance, the death of a king by marsh fever would now excite some sensation—"yet thus died Cromwell, one among hundreds; the death, indeed, not without note, but its cause not esteemed out of the ordinary course of mortality." In England, observes our author, "that which has been diminished, has not been extirpated." The fens of Lincoln are not the rivals of Walcheren—nor is Romney equally pestiferous as the Pontine Marshes. Still, as travellers, as merchants, as voyagers, and as soldiers, we are interested in the salubrity or insalubrity of all parts of the world, as well as of our own islands. Dr. M. thinks we may take the average of life among ourselves at 50; in Holland, at 25; in some districts of France, at 22, 20, 18—so little is the chance of life.

"Let us turn to Italy: the fairest portions of this fair land are a prey to this invisible enemy, its fragrant breezes are poison, the dews of its summer evenings are death. The banks of its refreshing streams, its rich and flowery meadows, the borders of its glassy lakes, the luxuriant plains of its overflowing agriculture, the valley where its aromatic shrubs regale the eye and perfume the air,—these are the chosen seats of this plague, the throne of Malaria. Death here walks hand in hand with the sources of life, sparing none: the labourer reaps his harvest but to die, or he

wanders amid the luxuriance of vegetation and wealth, the ghost of man, a sufferer from his cradle to his impending grave; aged even in childhood, and laying down in misery that life which was but one disease. He is even driven from some of the richest portions of this fertile yet unhappy country; and the traveller contemplates at a distance deserts, but deserts of vegetable wealth, which man dares not approach,—or he dies." 7.

The above is a fair specimen of the better kinds of Dr. Macculloch's style and manner—we shall have occasion to point out some less favourable specimens as we proceed.

The walls of imperial Rome cannot keep out this enemy of human life. It enters with the Roman into his chambers, and stalks through his streets—nay, "the hour is impending when the Eternal City will cease to be—when it shall submit to that fate, which has been the fate of proud Nineveh, and Babylon, the queen of nations."

Sicily, Sardinia, Greece, are grand seats of this destructive production, "while, in tropical regions, it is to fall by thousands and tens of thousands, the summer harvest of Death walking hand in hand with that of the vegetable world." England herself is far less exempt from malaria than she is supposed to be. Speaking of our participation, as soldiers, in the malaria of other countries, Dr. Macculloch rises into the sublime, or even the terrific.

"It is disease, not the field of action, which digs the grave of armies; it is Malaria by which the burning spirit, fitted for better things, is quenched, and in the coward's bed of death. This is the Destroying Angel, the real pestilence which walks at noon-day; and to which all the other causes of mortality are but as feeble auxiliaries in the work of destruction." 9.

Most of his readers will agree with our author, that "it is *not* the *field* of action which digs the grave of armies." The idea of a *field* digging a *grave* is more incongruous than that of a *grave* digging a *field*. If Dr. M. had substituted the *sword* for the *field* of action, the metaphor would have been highly poetical, and without any thing to shock the ear. We think these little incidental criticisms are fairly called forth, when we see a constant attempt at eloquence, and sublimity of ideas.

Dr. M. alludes to a characteristic feature in the moral character of those who inhabit malarious countries—namely, their firm belief in the healthiness of their native air and soil. Walcheren is instanced as an example. We can positively state that the inhabitants of Walcheren did *not* "repel with indignation the charge of unhealthiness which was brought against their beloved birth-place," by our troops on the late fatal expedition. We were there from the beginning till the end of the expedition, and can vouch for the fact, that the inhabitants considered the autumn of 1809 as a very sickly season among themselves, and remarked that this was the case at intervals of various duration.

Dr. M. offers nothing new, when he under-

takes to prove that the existence of a marsh is by no means essentially necessary for the production of febrile miasmata. Every naval and military surgeon knows, and many of them have demonstrated in their writings, that malaria will often arise from ground parched with heat; from the summits of mountains; the rocky bottoms of ravines; and the densest forests. We need not refer to the writings of Lind, Jackson, Blane, Burnet, Johnson, Dickson, Fergusson, Musgrave, and hundreds of others, for proofs of this assertion.

CHAP. II.—*Nature of the Evidences respecting the Production of Malaria.*

In this chapter, our author is conscious that he is "compelled to resort to proofs of some delicacy, and to appeals to an experience, for which, be it received as it may, he must be very often himself responsible." We are disposed to grant him every degree of liberal indulgence on these delicate points. We agree with him that it is a fundamental fallacy in this case to limit the power of producing malaria to those soils or situations *only*, where intermittent or remittent fevers are found to prevail. Such mistake, we grant, is very common in the minds of "imperfectly educated practitioners," of which, however, the number is rapidly diminishing.

This fallacy set aside, the real conclusion (observes our author) to be drawn is—"that, wherever *remitting* fevers or fevers of *whatever nature*, that are *not* contagious, as well as dysenteries, are produced, the proof of summer malaria is as complete as if the same soils had, in spring, produced ague." With some modification, we can have little difficulty in subscribing to this doctrine; for we have invariably maintained that all endemic, as well as epidemic fevers, were owing to something emitted from the earth—rather than to things generated in the air. This last is merely the vehicle by which the miasma is suspended or wafted about, like the mineral impregnation which mingles with the water of spring or stream.

The careful observer will often perceive that there are certain determinate places, without any marshes, where fevers are almost annually prevalent; while other places in the vicinity are almost wholly or nearly exempt. In the *former* localities, (Dr. M. avers,) some one of the various circumstances of soil, hereafter to be pointed out as productive of malaria, will, on careful inquiry, be found.

A more delicate proof may be drawn, he thinks, from the fact, that some localities are known to be *unhealthy*, as compared with other neighbouring places.

"Thus it is a vulgar remark, that in certain houses or places, a family is rarely without some sickness; or, to use the strong but coarse language in which it is generally stated, 'that the apothecary is never out of the house.' It is almost equally familiar, that families, which had before been healthy, have become the reverse on changing houses or situations; as, in the opposite cases, that they

have recovered health by change of residence. Of such facts as these, there is no observer who must not be able to recollect numerous examples." 19.

The laxity of reasoning among medical men, as to the causes of these local peculiarities, is strongly censured by Dr. Macculloch. But he should be merciful, and recollect that all men are not such acute philosophers as himself.

"To anticipate, but no more than is here necessary, what must shortly be said on the subject, if a gravelly soil is healthy, it is because its easy drainage prevents the growth of that particular vegetation which is the cause of malaria; and if a clayey soil is the reverse, it is because, by lodging superficial water, it generates, however partially, those marshy or undrained spots, or wet woods, or moist meadows, which are the sources of this poison, and, consequently, of the various diseases confounded under the vague term unhealthiness." 21.

It is to be remembered, however, that gravelly soils often contain spots generative of malaria; while large tracts of clayey soil are often as dry, and, therefore, as healthy as the most porous ones.

Dr. Macculloch thinks that, if practitioners will attentively observe the phenomena presented in what are called unhealthy situations, they will find either annual fevers, or bowel-complaints, cholera, head-aches, periodical or irregular rheumatism of the face or head, tooth-aches, sciatica, tic douloureux, or other varieties of neuralgia, bilious affections, or some, or many, of what are vaguely denominated nervous complaints, the prevailing disorders of the said localities. And even if these should be absent, or the poverty or other circumstances of the people should prevent their making them known to the physician, still the sallow complexions, the languor, the irritable tempers, or the melancholy character, of individuals thus unfortunately situated, will, Dr. M. thinks, afford the observer sufficient evidence of the operation of malaria on the glandular viscera and general health.

Our author next adverts to a test of a very delicate nature, namely, the liability of persons, who have once suffered from malaria, to become again affected, when exposed to even a very trifling degree of the same. Thus, the sufferers of Walcheren are known to have experienced relapses in various parts of the world, and at various periods, after that fatal expedition. "Hence, therefore, when we find that such a person has experienced a renewal of his disorder, from communication with a place otherwise suspicious from its nature, it offers as convincing a proof as can be desired, that there, malaria is produced or producible." In such situations, the source of the poison may be so circumscribed, as not to affect people in common health; and this negative argument is often offered in favour of the local salubrity. Speaking of the unpleasant task of pointing out sources of malaria, Dr. M. observes:—

"My duty, as it is my design, is to make them known; let *he* (him) who has the power of convincing mankind that they have been in error, and that they are ignorant, undertake the other task. But time effects what man cannot; and hereafter, perhaps, an English gentleman will be as much surprised that his neighbour should dig a sleeping canal before his door, as that his feudal ancestor should have built his castle in a marsh, and enclosed it within a putrid moat.

"To suggest that he who does this is sowing the seeds of disease, that he may reap the fruit of fevers and apothecaries' bills, is to excite the smile of superciliousness or contempt; as he must long yet submit to be the object of both, who would try to convince mankind, that the pond which has been constructed for a few gold fishes, or the river which meanders through the woody valley, is a death-spring of diseases, or that the fevers and the tooth-aches which are the torments of his family, the ailing wife, who is own torment, and the sciatica which is the torment of his poorer neighbours, are the produce of a few bunches of rushes, or of a splendid display of water-lilies." 31.

We fear that there will be many who will look upon this doctrine as inclining a little to the visionary. Something must be allowed, however, for the style and ardour of language employed by the author. The matter should not suffer on account of the manner. Dr. M. considers, and not without probability, that the obstinacy of chronic diseases is often owing to the repeated application of their cause (malaria) rather than to any change of structure in the organs affected.

CHAP. III.—*Soils and Situations most productive of Malaria.*

This chapter is comparatively short, because it treats of localities, which are generally allowed, or, unfortunately, proved to be prolific of malaria. Marshes claim the first place, and salt-water marshes, especially in the warmer countries, are very properly considered by our author—and, indeed, by most other good observers, as equally pernicious with the fresh-water pools. The salt marshes of Dol, in Normandy, are notoriously productive of intermittents, so that scarcely an inhabitant escapes. The same is seen on the French shores of the Mediterranean—in the Adriatic—in Greece—Italy—Sicily—Sardinia—Spain, and a hundred other parts of the world, including both hemispheres of the tropics. Ever since the days of Sir John Pringle, indeed, it has been admitted, that an admixture of salt water with fresh augments the disposition to the putrefactive process, both in animal and vegetable matters. There is little doubt entertained by medical voyagers and observers, that the shores of coasts, and more especially of rivers, washed even by the sea-tide, are amply productive of the poison in question. The banks of rivers where palms and mangroves abound, have been always observed to give origin to bad fevers, fluxes, &c.

"There are few tracts in England more productive of a Malaria, which is even of a virulent nature, than Heron bay and the river banks in general about Reculver, where the water is salt, and the whole is covered twice in the day. The same, indeed, is true of so many parts of England, that the enumeration would be equally tedious and superfluous. Be the truth, however, what it may, in this case, it will be always the most safe belief to adopt the opinion, and to act on it; as the philosophical evil of the error, if it be one, bears no comparison to its value as a practical security." 39.

That woods and jungles, in hot climates, give origin to miasmata of the worst kind, is well known to all medical men; but some doubt may be entertained as to their insalubrity in Europe. Dr. Macculloch, while he suspects the said localities, admits that he has no positive evidence which he is able to bring against them. He thinks there is strong reason to believe that close and wet woods generate malaria in this, as well as in the warmer countries of Europe. Certain woody districts in Sussex and Kent produce both intermittent and remittent fevers—at least there is no other assignable cause. The same may be said of some parts of Hampshire and Essex, as about Epping Forest, for example.

On the other hand, we have positive testimony that lands which were healthy when covered with wood, have become extremely unhealthy when cleared and cultivated. This has been often observed in America. It may be accounted for in several ways. The woods keep off the direct rays of the sun from the wet earth, and the trees may prevent the propagation of the miasmata through the air. When the woods are cleared away, and the earth turned up, the sun powerfully exhales the miasmata, and the winds waft them about in all directions. Reversely, it follows that the planting of trees will sometimes check the production of malaria, by protecting wet lands from the action of the sun. But it requires much circumspection in deciding on planting or clearing grounds with a view of rendering them more salubrious.

"To say that rice grounds are productive of Malaria, is equally to state a fact notorious to the whole world; while the causes, consisting in a succession of inundation and drainage, approximate them in character to swamps and marshes, however obscure the immediate operation of either in producing this poison may be. How extensively Italy suffers from this cause, it is quite superfluous to say; since the mortality in Lombardy, and elsewhere, arising from it, is matter of daily observation, even to the most incurious travellers. And the same is true of Greece and Sicily, as it is generally of Europe, wherever this grain is cultivated." 46.

Such then are the principal and undisputed sources of malaria, in the description of which it was unnecessary to be at all minute.

CHAP. IV.—*Of Soils and Situations less con-*

spicuously productive of Malaria, or as yet unsuspected of it.

This is the chapter which contains many views of the author which will, in all probability, be disputed by the critical tribe. We shall endeavour, therefore, to make our author as clearly understood as the limits of a review will permit.

To begin with a marsh or swamp. It is supposed that a certain *extent* of such locality is necessary to the production of disease. "This is an error; and it must be classed among the dangerous ones, as being productive of a false security." This is merely an argument as to the plus or minus of a poison. If a large tract of marsh produce a given quantity of malaria, this sum total must be an union of all the portions generated by its parts. And if, as is generally supposed, this poison be the chemical produce of vegetables acting on water, or water acting on vegetables, "then must every plant and fragment of a plant contribute its share to the deleterious substance." Dr. M. also observes that, as there is a certain analogy between malaria and the matter of contagion, and as we know that a quantity of the *latter* which is quite incognizable by the senses, and insensible to every chemical test, will produce its peculiar disease, so we have no reason to doubt the same effects from malaria.

"It would bear that analogy in this point, which it does to contagion in so many others, if a small quantity were as efficacious a poison as a large one; and there are reasons for supposing, practically, that this is the fact, since it is matter of observation, that a minute's exposure to Malaria, a single inspiration probably, and of a poison which must be far more diluted than contagions can ever be in the same circumstances, is sufficient to excite its fever, and, very notoriously, to re-excite it in those who are subject to that morbid sensibility derived from former or habitual fevers." 54.

From this and other matters of fact or observation, our author thinks he is justified in concluding that—"the quantity of malaria necessary to produce its peculiar disease or diseases, is undefinably small, and probably extremely minute." Although we are inclined to give our author every indulgence in the investigation of such difficult matters, yet we are here forced to dissent from him on a most important and fundamental point. It is universally acknowledged, that we are totally ignorant of the nature of marsh or human effluvia, and only know them by their effects. The observation of these effects would induce us to believe that the *quantity* of human contagion—for instance, of syphilis, small-pox, &c. is of little consequence, whereas the *dose* of marsh miasma is every thing. A man may stay twelve hours on the source of a most deadly miasma with impunity; but let him stay four or six hours more, and death will be his lot. This has been proved at Batavia, the Pontine Fens, &c. During the day there is

little danger; but in the night, the poison is almost certain of its victim. To the following passage we must also object.

"Could this admit of doubt, or should those who have made no observations, or who are incapable of observing, choose to deny the well-known facts now alluded to as evidence, it would be proved by the great distance to which Malaria travels through the air without losing its poisonous quality. Not to dwell here on examples which must be adduced hereafter, it is quite familiar that from any known and often very limited spot, this poison will proceed through the air, or on the winds, to distances of three or four miles, exciting as much virulence as in its native marsh. This, to quote a familiar domestic example out of a hundred that might be adduced, occurs on the hills of Kent, far from the marshes of Erith, Northfleet, or Gravesend; and it is easy to see that whatever was the body or quantity of Malaria in the original place of its production, or whatever portion of atmosphere it occupied over the few acres by which it was produced, it must often, in such a course, have been diluted to a degree so incomprehensible, that while we can only wonder how it should exist at all as a distinct substance, or a chemical compound, even more must we be surprised that it should be capable of producing its peculiar diseases, with an activity as great, and often greater, than it did at the very point of its birthplace." 55.

How does the above comport with the well-known fact that our ships moored close along the banks of the Scheldt entirely escaped the fever, while the soldiers quartered within half a cable's length of them were all affected with the endemic? The same has been observed in hundreds of other places. We do not say that miasmata are not carried along by the winds; but we do maintain that they are *weakened* by the dilution, in as nearly a ratio to the distance carried as possible. How is Dr. M. certain that the hills of Kent do not generate miasmata, seeing that the hills in other parts of the world are not exempt from such productions? Nothing is more common than marshy spots and even stagnant water on the tops or declivities of hills—while at their bases, springs of water and plashy grounds are invariably found. The marshes of Erith, Northfleet, and Gravesend, may not then be guilty of producing fevers on the distant hills of Kent. We doubt whether the following conclusions will be admitted by the professional public, although Dr. M. seems convinced that he has reduced them to a kind of arithmetical certainty.

"The conclusion is obvious; and there is nothing in it which seems to admit of dispute, since it is almost a question of arithmetic. If the produce of a hundred square feet, or acres, or of any scale and number of parts, can, under a dilution of one thousand or ten thousand times, excite disease, then must, in the inverse ratio, the produce of the one-thousandth, or the ten thousandth portion of that space be capable, before dilution, of producing the same

effects; or a single blade of grass acting on water (if this be the cause) may be as efficacious as an acre; supposing, of course, that it is actually applied to that part of the body which can suffer from its action." 56.

Dr. M. next proceeds to an analysis of what a marsh or swamp is. This varies much in its obvious aspect, according to the nature of the plants which form its vegetation—these last being considered, however diversified in kind, as mere vegetables, living or dead—since we do not know that the different kinds of vegetables make any difference in the kind of miasmata, though this is not improbable, considering the great variety of elements which enter into their composition, and the different actions which they induce in the animal body. This, however, is all conjecture, and we must trust to future investigation of the subject.

"But to pass from this; the essential character of all marshes and swamps, as far as we yet can decide, is, that the land should be partially inundated, that it should be dry in some places and wet in others, or that pools and dry spots should be intermixed, or that it should be boggy and soft from the mixtures of earths and decayed vegetables with water, or that it should be subject to peculiar alternations of moisture and dryness, sometimes amounting to absolute inundation in the first case." 61.

Dr. M. remarks that the miasma in question is not produced "by the mixture of decomposed and subcarbonized vegetable matter and water, since it is notoriously not produced by dead peaty bogs, or by *peat which carries no vegetation*." If Dr. M. had been locally acquainted with some of the immense bogs in Ireland and Scotland, he would know that, on their surface, in the Summer time, there is the most luxurious vegetation, most part of which decays and dies in the Winter—yet *there*, an ague or intermittent of any kind is rarely seen. In such localities, then, there is abundance of living vegetable matter in contact with water during the Summer—yet no miasmata are generated. In Winter, the vegetation dies—and still no morbid effluvium is generated. The conclusion which we would draw from this circumstance is this, that, during the *Summer*, the surface of peat bogs is all *life*, and consequently the pabulum of miasmata does not exist—while, in *Winter*, when the decay or death of vegetation takes place, the *temperature* is unfavourable to the production of the said malaria, and thus the people residing on or near these bogs are fortunately preserved from any insalutary influence.

Recurring to the case of marshes, our author labours to show that this peculiar contact between a living vegetable, or a vegetable in an incipient or somewhat advanced state of decomposition, and water, does take place in many situations that are not marshes, in the popular, or in any sense of the word;—and, consequently, (if this be granted) that a thousand places hitherto unsuspected, are capable of exciting the disorders which result from malaria.

Now, that this peculiar state of vegeta-

tion, not only as to the appearance and character of the soil, but as to the mode of growth and death, and the very nature of the plants themselves, does occur in numerous situations that are not marshes, is the point to be proved, and is a point indeed that will require no proof to almost the most superficial observers; no proof assuredly to botanists, whatever it may to medical men; not often even to the observant inhabitant of the country, whatever it may to the limited man of towns and cities. If the botanist will recognize the spots in question by the nature of the plants which attach themselves to such soils, if the growth of an *Iris*, an *Equisetum*, a *Hydrocotyle*, points out to him what the farmer sees, though less acutely, in tufts of rushes, or traces by the coarseness of the pasture or the canker of a tree, it is the latter who will know every spot of land about him which asks for drainage, where he to whom these pursuits are strange, will seek in vain, even should he, as a physician, be engaged in investigating this very question in a medical view." 64.

It is a popular opinion, he observes, that the rushy pools and petty swamps so common in high moorlands, are innocent. The fact, he avers, is not so. He has seen intermittents in Wales, and at considerable elevations, in those very situations. The following is an example.

"A considerable body of labourers were employed in excavating a pond on a moor of this nature, situated about a thousand feet above the level of the sea; and in the course of the work, within a very short time, nearly one half were incapacitated by the ague." 67.

Dr. M. is convinced that the minute marshy or swampy spots which occur in thousands of low situations, whether on commons, near woods by road sides, or in innumerable other places where they scarcely or never attract notice, are similarly productive of malaria, though their limited range of action generally renders their power in this manner insensible, unless when houses happen to be erected in their vicinity. In how far meadows are capable of producing malaria, he is not completely able to say. Being often intersected by drains and ditches, the miasmata may be generated by these last, instead of by the including land itself.

"I cannot hope to clear this question by an exact definition; but, taking the term in its usual lax sense, it appears unquestionable that there are many tracts of meadow, or of alluvial land, not marshy, and often not intersected by ditches, at least in a conspicuous manner, which are the sources of Malaria all over Europe." 69.

Such, he observes, is the case with the alluvial tracts at the entrances and sometimes at the exits of the lakes of Switzerland and elsewhere, and in places innumerable where there is no proper marsh, nor even an approach to such a character, but where the prevalent diseases must be owing to malaria.

Volney, who could have had no theory to support, has averred, while travelling through America, that every valley in the country

which he visited, produced the fevers of malaria, enumerating among the sources of this poison, not only marshes and woods, but rivers, mill-ponds, &c. Reverting again to the subject of meadow land, Dr. M. observes, that a fruitful source of miasmata may probably be found in "that drying, during Spring and Summer, which follows the moist or wet condition of such meadow lands, as they are left by the Winter rains." A striking example is given in the lands about Fontainebleau, at the junction of the Yonne and the Seine, notorious for the "*fièvre du pays*," so injurious that few escape intermittents or remittents over a considerable tract, for which there is no other ostensible cause but the meadow lands, which are inundated or soaked in Winter, and exsiccated in Summer. Wherever, therefore, the temperature be sufficiently high, he thinks this will be found a productive source of malaria in all countries.

If some great tracts of meadow land in England have been recovered by drainage from a state of marsh, and are now as dry as the ordinary low lands of plains and valleys—and if these localities still produce malaria and its consequences, it is another point of evidence against the salubrity of meadows generally.

"This is true of the meadows which border the Thames, not only beneath London and through their whole extent, but above it; which, though often retaining the name of marshes, because once marshy, are now as dry as the common meadow lands of inland valleys and plains. It appears to be the fact also in many parts of Cambridgeshire and Essex, and, among others, in the vicinity of Waltham Abbey; as it also is in Kent, in the Isle of Thanet, in Somersetshire, in Lancashire, in Huntingdonshire, and far more commonly indeed than it is necessary or convenient to enumerate. Thus it also was, even in the Carse of Gowrie in Scotland, until that great tract of alluvial meadow was brought into universal cultivation; and this may perhaps serve to prove that the meadow land itself, and not the ditches, was the cause, because the latter remain, while the grass has been succeeded by almost universal crops of grain. And it will be found, in confirmation of this, in France and in Flanders, and probably far wider than I now know, that where tracts bordering the same river, or in any other respect exactly similar, whether in soil or situation, are, respectively, cultivated with grain or kept in grass, there the production of fever or of Malaria is correspondent; occupying the uncultivated lands so as to produce what is popularly called the *fièvre du pays*, as if it was a necessary part of the order of things, and flying from those that have been ploughed for a grain cultivation." 75.

Dr. M. observes, that it is a rooted opinion in England that there can be no malaria on the banks of a running stream. As far as mountain torrents are concerned, this is probably true; but, where rivers slowly meander through low grounds, we must not trust to the mere motion of the water. We know, indeed, that the banks of rivers, in hot climates, are truly

pestiferous. In those portions of rivers where the tide of the Ocean reaches, as, for instance, along the Thames as far as Richmond, Dr. M. thinks that malaria must be generated by the exposed mud, at each recession of the tide.

"Whatever doubts may still exist as to rivers in general in our own country, in this case, there is no reason whatever to doubt that such streams as the Ouse, the Lee, and all others flowing with similar difficulty through fertile meadows and with a flat vegetable margin, are productive of Malaria, because the diseases which attend it are common in all those situations." 79.

Dr. M. considers those small streams which flow through gentlemen's grounds "almost like artificial canals," bordered by thin and grassy margins, as productive of disease, however the popular opinion may be against such insalubrity. The same observation applies, *a fortiori*, to canals, which are intermediate between a sluggish river and a stagnant pool, their margins possessing, or being capable of possessing, all the essential qualities of a marsh, "as a diminution of their waters may expose mud impregnated with vegetable matter." This, the Doctor observes, is the point which we must always have in view—"it is the analysis of the whole question."

"If it is not putrefying mud, it is the marshy spot, the peculiar vegetation, or death of vegetation, carried on at a certain point of vacillation between earth and water, which is the generative cause; and, while this may exist in a hundred different characters of ground or situation, and while further it is not essential that bulk or space should be present, it is easy to see that the business of investigation is, in reality, reduced to a very simple principle; for those, at least, who are gifted with the powers of observation and generalization. Let this fact be ascertained by a due examination of any spot, and the probability, at least, of Malaria is established: let it further be ascertained that certain diseases do belong to those situations, taking care also to prove that they are endemic or local, and the fact of its production is determined." 82.

Our author again passes in review the insalubrity of canals, ditches, drains, privies, moats, &c. Speaking of the late fatal endemic which scourged the unfortunate inmates of the Millbank Penitentiary, he says—"not the slightest doubt ought for a moment to have existed, either with respect to the cause or the disease." A remedy, he remarks, was sought "by letting in that malaria which it should have been the object to exclude or else destroy." The ditches surrounding fortifications afford our author, of course, ample field for supporting his favourite doctrine—especially the fortifications of the low countries. The following passage in respect to lakes, is not devoid of interest.

"But it must also be said in explanation, (a view which is important, as it concerns all waters of this nature, even to pools,) that in France, it is supposed that the Malaria is not solely produced by the vegetating marsh, but

is disengaged from the mud which the summer leaves dry, (a fact which I must notice again) and that it also escapes from the bottom and through the water, accompanying the air which is so notably extricated in those cases. And in confirmation of this, it is said, that while such pools retain a considerable depth of water, or whenever their banks are steep, no Malaria is produced, but that it appears in the reverse cases, or, either on the diminution of the water in depth, or on its retiring from the shores. The same facts, I should observe, have often also been noticed in the West Indies; while a very strong case, illustrating this particular cause, is stated by Senac, in France, where, in a town previously unaffected by fevers, a violent epidemic was produced, in consequence of an unusual evaporation which exposed a large portion of the bottom of a lake. From these facts it is an obvious inference, that in warm climates, at least, whatever may be the case in our own, tranquil or stagnant water is unsafe in any form, and that a vegetating margin is not rigidly necessary to its pernicious qualities; though it cannot be doubted that the evil is materially diminished by cutting off this additional source of Malaria." 98.

It appears that the present alteration in the canal of St. James's Park, is the suggestion of Dr. Macculloch—but the Doctor's fears are far from being hushed by the amendment which he proposed—"since it is notorious for the abundant produce of aquatic plants, causing in Autumn an even insufferable stench."

"Whether the pond in St. James's square also, forming so refreshing a receptacle for its statue, claims the same English exemption or not, must be decided by Monfalcon; as I am not courageous enough to think that such an Italian substance as malaria can exist in the centre of the English capital." 101.

Dr. M. remarks, that "to prove that mill-dams, though transmitting large streams, ought to be injurious, from the frequently marshy nature of their margins, would be to repeat what has been said before, respecting the *priori* proofs on this subject in general." About the iron district of Glamorganshire, he informs us, there are numerous large mill-dams, for the supply of machinery; "and there is not one of these, in the lower grounds, which is not notoriously attended by the endemic ill-health of all the immediate residents and visitants, consisting in the diseases already mentioned." Of these, neuralgia, he states, is a very common form. These local exceptions to the general health of the surrounding hills and dry places, are peculiarly remarkable, and have attracted the attention of the inhabitants themselves.

As it is incumbent on medical men to attend to medical topography, and thus to put the doctrines of our author to the test of experience, we shall dwell more on localities than we otherwise would have done, in order to excite the attention of our readers to this important subject of investigation. A mill-dam, he says, at Southend, near Lewisham, affords

a striking example, though on a small scale, while it is also an instance applicable to fish ponds, and other kinds of still water similarly circumstanced.

"Here the poorer inhabitants in particular, are notably subject to intermittent as well as autumnal fever, while they bear marks of glandular visceral affections, and are reported to die of the consequences of those disorders. To have seen the fit of intermittent invariably produced in a susceptible individual by an approach to this pond, hundreds of times, and always within a stated distance of time from the approximation, completes an evidence which cannot be controverted." 105.

In farther illustration, Dr. M. instances the valley of the Ravensburn, with the communicating low lands, including the villages of Lee and Lewisham. There is a peculiar physiognomy, he remarks, attached to all such places, which renders it easy to distinguish them.

"I may here add another instance, from the mill-dam of a paper-mill in Hertfordshire; after the formation of which, the workmen became subject, in a most serious degree, to remittent fevers, which were, before that, unknown; and as the ground in this particular instance resembled that of an ornamental park, as did the water itself, it may suffice to prove what I have advanced on that particular subject; although it would be easy to confirm this by analogous instances adduced from many of the dressed pleasure grounds ornamented by water, which skirt the Thames, near Walton and Chertsey, and which occur also in a hundred other places: the produce of a well-known improving gardener, or else of his progeny; to the demerits of whom, as the sources of an endemic disease of English landscape, far, very far yet from being extirpated, an eruptive contagion blotting our fair island, it is no small addition that they have, in founding ponds which their vanity mistook for rivers, and in converting rivers into Dutch canals, brought the intermittent to our doors under cover of the breeze of the violet, and formed pest houses of fever where we study to retire for coolness from the heats of the autumn. This is to manufacture a Batavia, in defiance of nature; to court disease through deformity and expense; the evil less, it is true, but of the same kind, and incurred as certainly." 106.

The above is another fair specimen of the style and manner of our author, where he aims at being peculiarly impressive through the medium of unconscionable sentences—a single one of which just occupies a *page* of the original work in the foregoing quotation.

Dr. M. here instances a spot in a high, and formerly healthy part of Hampshire, where a clear and quick stream was dammed up, not long ago, for ornament and use. The immediate consequence was, the production of evening mists, before unknown—and the result was, autumnal fevers. Dr. M. never lets an opportunity slip of levelling a sarcasm at the *country apothecary*, though a better acquaintance with that class of medical practitioners would have taught him, that they *think*

just as much as, and probably more than, their brethren in towns and cities.

"A French or an Italian *physician* would be at no loss here in deciding; but the *English apothecary*, having no term but typhus for a destructive fever, decides accordingly; never questioning himself as to the origin of the contagion of which he dreams, nor ever recollecting to wonder why it should not spread to the attendants, when the patient is covered with petechiæ; and thus the public goes on, creating more mill-dams, more fish-ponds, more fictitious rivers, and, after the models of Brown, more fevers." 107.

We are not among those who flatter, for base purposes, one class of the profession at the expense of another. There is a mixture of ignorance and intelligence in every class—and all sweeping conclusions, that would represent one class as dolts and another as angels, are necessarily unjust, as well as ungenerous.

Here our author quotes an instance, (doubtless in his own person) where the recurrence of an intermittent fever, in a susceptible subject, was caused repeatedly, "*by merely entering a garden, containing a pond of the fashion of King William's day, dedicated to gold fishes and river gods.*"

With such a rare degree of delicate susceptibility, Dr. M. would prove a most valuable miasmometer, and as such, might render essential service to the government, as well as to families, in ascertaining the salubrity or insalubrity of various localities, before they are selected for public establishments or private residences. Dr. M. relates another instance, which happened at Woolwich. There was a small pond occupying an old gravel-pit on the common, close to a house belonging to the late Dr. Hutton, and occupied by General Stehelin—its whole extent being but a few square yards. It was remarked for a long course of years, that the inhabitants of this house were perpetually harassed with agues; and it was not until this pond was destroyed by improvements in the common, that the disease disappeared—for ever. Dr. M. has no doubt that the occurrence of ill health, in numerous places where the gravel pits of commons are filled with water, is the consequence of this very cause; and that, in reality, those situations about London, &c. so often selected for the supposed salubrity of their gravelly soils, "are very general, and not less unsuspected causes of ill health." What will the rosy-cheeked inhabitants of Putney-heath, Wimbledon, and Hampstead say to this? There is no doubt, however, that, in all these places, *agues* have actually shown themselves this very year (1827); but then the said complaints have prevailed in every county of England, and in almost every street in London. There is something more, in our humble opinion, than stagnant water or decaying vegetables to be taken into account in explaining the production of epidemics. We apprehend that Sydenham was not far wrong, when he suspected an invisible morbid agent, springing

from "the bowels of the earth." Still there can be no doubt that the causes forming the subjects of investigation in this Essay, do ordinarily work the effects ascribed to them by Dr. Macculloch.

To the inhabitants of London, our author could easily point out numerous places, even in their own vicinity, illustrating these several causes, but the remarks already made will enable every medical man at least, to investigate the topography of his neighbourhood—his autumnal practice being a good index of the quantum of malaria disengaged from the soil at the time.

It has often been noticed, in various parts of the world, that the breaking up, for the first time, of pasture lands, is attended with sickness. The evidence on this head is as abundant as it is unquestionable. Volney, Nash, and fifty other authorities, might be cited in proof.

"Why this should be the fact, if it cannot be very precisely explained, is not at least more difficult than most of what else belongs to this subject; since there is a quantity of vegetable matter killed, and therefore submitted to decomposition; and it would be well worth the trouble of those whose local situations give them the means, to inquire whether this, and many other analogous agricultural processes, now little suspected, are not the causes of the fevers which sometimes appear in rural situations in such an inexplicable manner, when these cannot be better accounted for by stagnant waters of various kinds, or by such neglected spots as I have here been pointing out. The remark is of value, be the solution what it may; because the remedy will be found in breaking up such lands in June, or in May, if the summer be the necessary period, or, what is preferable, in the middle of winter; since the decomposition will then take place at a time in which experience has shown that Malaria is scarcely generated in our own country, nor indeed, generally, in Europe. In the case of lands recently recovered by drainage, this precaution is peculiarly deserving of attention, because in this case the danger is greatest; and the same is equally true of woods, the mere felling of which sometimes disengages or produces Malaria, as is a much more certain consequence where, as in America, and as I have elsewhere noticed, these woods are broken up for cultivation." 113.

The same observation applies to drainage as to tillage. A swamp may be too wet to produce miasmata—and a certain drainage may just bring it into that state which is peculiarly favourable for the extrication of the unknown poison. This appears to have been the case with the Campagna di Roma, as far as the facts can be ascertained by comparing the different accounts of Italian writers. The most pointed instance, however, is that of the marsh of *Chartreuse*, near Bourdeaux. A succession of bad fevers, before unknown, showed themselves immediately after the drainage of the above marsh, first in that part of Bourdeaux

which lay nearest to the land reformed, and afterwards through the whole of the town. These fevers lasted for many years, and proved so severe in 1805, that 12,000 people were affected, three thousand of whom died in five months!

"It is not difficult to understand that a swamp in which the water is so deep as to impede the growth of as many plants as a drier surface would carry, will produce proportionally less of the poison in question; and that a similar diminution or under proportion of Malaria will attend such a tract of land if it should contain many pools or spots divested of all vegetation. In such a case, we can conceive a certain state of drainage, such as to increase the vegetating surface, without being at the same time complete enough to check the production of Malaria; or a small quantity of poisonous marsh might thus become a large surface of wet and noxious meadow land." 115.

Dr. M. touches on the obvious difficulty resulting from the well-known fact that malaria, as ascertained by its consequences, does obtain in some places perfectly dry and gravelly. Our author says that, before we admit this to be a fact, we should be very certain that, in these instances, the malaria is not transported from other places—or that, in such dry places, there are not ditches and drains left from the operations necessary in rendering the ground dry. This does not, in our opinion, clear up the difficulty, as there are unhealthy places where none of the latter exist, and where there is no ostensible source of morbid effluvia within any reasonable distance.

In a chapter respecting some anomalies and inexplicable instances of salubrity and insalubrity, we were rather surprised to read the following passage:—

"But there is one mystery for which I can conjecture no solution, while it rests on great authorities, and while every imaginable circumstance is present that ought to render the land in question one of the most pestiferous spots under the sun. It is a collection of jungles and woods and marshes and rivers and sea swamps, and it is a flat land under a tropical sun, and it is the land of monsoons; and yet it is a land where fevers are unknown. And this land is our new settlement of Singapore. I dare not attempt to controvert such testimony, and must try to believe what I cannot understand: but others may, for aught I know, be inclined to suspect that some favouritism, not perhaps inexplicable, has dictated this report." 138.

We can relieve our author from the dilemma in which this new settlement has thrown him: Singapore is *not* a collection of jungles, woods, marshes, rivers, and sea-swamps; but a cluster of high rocky islands, in the midst of an azure sea, and under a sky that is seldom darkened by a cloud. There are no marshes or swamps in the neighbourhood, and the change of the monsoons is not marked by those storms and tornados which occur in other parts of India. The straits of Malacca and Singapore were always looked upon as

the Montpellier of India. Dr. M. may therefore consider that one of the horns of his dilemma is broken off.

Our author next proceeds to notice certain other sources of malaria than marshes or marshy grounds; and among these the putrefaction of vegetable matters takes a leading rank. The first instance quoted is the process of soaking hemp and flax, the offensive nature of which is well known.

"Of pointed facts beyond number, related both in France and Italy, we find in Lancisi, that numerous severe epidemics in the latter country have been traced to these operations, and, among the rest, a noted one at Ferentino, and another at Orvieto, which lasted many years. In the former country, out of similarly numerous cases, severe intermittents broke out in the plain of Forez in 1823, after October, (a very rare occurrence,) and were traced to this cause; and we have the assurance of M. Bourges, that it is invariably pernicious, while he describes one very marked case where fevers occurred in a dry, sandy, and otherwise healthy and elevated situation, being regularly renewed with the steeping and drying of the hemp; and disappearing when that season was over." 141.

In Germany, where this manufacture is extensively carried on, it has been often proved that fevers, and those of a very bad kind, are the result:—a fact which, Dr. M. thinks, tends to establish an opinion elsewhere broached, that the nature or severity of fever may be considerably dependent on the nature of the particular malaria, or the quantity in which it is applied. A more accurate train of investigation, he thinks, may probably detect many sources of malaria, of this kind, of which we do not now dream—as, for example, in garden dunghills and the like. He attaches some suspicion to the decomposition of the wood of casks in which water is kept on long voyages, or on shore. He sees no reason to doubt that the wood, in such instances, should generate malaria by its decomposition, as well as the woody elements of herbaceous plants in general. The same suspicion attaches more strongly to bilge water—especially where there is a mixture of various matters in the ship's hold, as, for instance, where there is a leakage of sugar. The writings of Bancroft, Dickson, M'Arthur, and others, have completely established the important fact that fevers of the most malignant kind have been generated by the action of water on various materials in ship's holds.

Among the intricacies and difficulties attending the investigation, our author has appropriately placed the comparative healthiness of ancient and modern Rome.

"The ordinary conclusions of natural history will determine, in the first place, that the site of Rome, as well as the surrounding country, must, at its foundation, have been a tract of woods, lakes, and marshes; and, that such a territory must have been productive of fevers, appears an inevitable consequence. In spite of this, the city flourished and increased,

while the surrounding country was also filled with a population distributed in hamlets and villages. The plain of Latium for example, which is now a desert, was, at that time and long after, rich and populous: and thus also the lake of Castiglione, now infamous for its pestilential air, was the seat of a powerful city which long resisted the arms of Tarquinius Superbus. The ancient Latium was situated near a marsh which is now one of the most destructive spots in this district; and the Romans erected baths beyond the Anio, in a place which is, at present, too hazardous even to be visited. The Lago di Giuturna was a favourite spot with the ancient Romans; yet in later times it rendered Castel Gandolfo uninhabitable, and was therefore drained in 1611, by Paul V. In the time of the Volsci there were twenty-three towns and villages in the Pontine marshes, of which Ardea and Lavinium were two. But as it is unnecessary to accumulate more of these specific facts, I shall only further remark, that history confirms what might have been inferred from general considerations, namely, that the country round Rome was in ancient times interspersed with what were called lakes, and which were, in fact, chiefly marshy pools; as must necessarily be the character of accumulated water in a country of such a form and distribution. And these tracts, which were then populous and flourishing, are now uninhabited deserts; although the lakes and marshes have comparatively disappeared, under different attempts at drainage, attended by various success." 165.

In respect to the Everlasting City itself, at that early period, the facts are similar, and the conclusions not less puzzling. The whole site of ancient Rome formed, as can scarcely be doubted, "a focus of malaria and fevers." Yet the city, as well as the surrounding country, increased rapidly in population. Thus the first census by Servius Tullius produced 80,000 citizens capable of bearing arms—while we find that Ardea alone, which now reckons 600 inhabitants, was then able to raise an army sufficient to resist Rome, and also to send a colony to Saguntum. Ostia, now inhabited by a single inn-keeper, became a flourishing city soon after its foundation by Ancus Martius. These are some of the puzzling facts which history has left us to unravel. Whether the production or the virulence of the malaria has increased in modern times—or whether the ancient inhabitants had means of resisting its influence which the moderns have not—are the questions which remain to be solved. It cannot, our author remarks, be safely asserted that at any period of the history of Rome, the city and neighbourhood were free from malaria and its consequences—indeed, there is strong reason, he thinks, to believe that it was as poisonous then as it is now—"though the apparent effects or the political consequences were less severe."

"It may be thought indeed, that as to some parts of this district, if not to all, the evil has really increased in modern times, not solely from the decay of agriculture arising from that

injudicious political management as to corn laws, so often blamed, and from other analogous causes as often discussed, but from geological changes as to the form of the land itself: and of such facts and their consequences, to a certain extent, there seems ample proof. The joint action of the sea and the rivers will, in the case of the Pontine marshes, easily explain a change on this important point, fully adequate to an increase of the evil: and reasoning of an analogous nature may be applied, under modification, to more inland districts."* 167.

Dr. M. thinks it not unimportant to remark that, as stated by Theophrastus, the plain of Latium was covered, especially towards the sea, by forests of laurel (bay) and myrtle of such a size as to be used in ship-building—"constituting, doubtless, screens to protect the country from the pernicious southern winds, and to check the propagation if not the production of malaria." Dr. M. thinks the ancients well knew the value of this expedient, and that much of the evil was thereby warded off—hence the sacred characters of groves, and the heavy penalties denounced against those who destroyed or injured them. This explanation we cannot help looking on as very fanciful. It is highly improbable that the Romans, at that time of day, knew much of the nature of marsh miasmata, or the means by which these mysterious agents could be counteracted or avoided, notwithstanding the observation of Pliny that groves absorb and destroy mephitic vapours. We think there can be little doubt that great changes in the climate (we mean territorial) of Rome and its vicinity have taken place within the last two thousand years. We see places entirely changed in the degree of their salubrity or insalubrity, within the short space of a few years, of which we may instance Prince of Wales' Island (in the Straits of Malacca) and St. Helena, in the middle of the Atlantic. Both of these places became very unhealthy a few years ago, though previously

* When we contemplate the millions of myriads of human and other living beings which have mingled with their mother dust in Rome and its vicinity, since the days of Romulus and Remus, it is not a very extravagant flight of fancy to imagine that almost every particle of earth under the feet of the present inhabitants, has been once endued with animal life, and therefore that the vegetables on which the Romans feed spring up from, and are nourished by the ashes of their forefathers! There is something in the thought which makes one shudder, and suggests a faint idea that Nature herself affixes an ultimate limit to the congregation and residence of huge multitudes of human beings in any one spot of this earth's surface, however favoured may be the locality in all other respects. The fond aspiration of "ESTO PERPETUA," then, is one of the many "vanities of human wishes," at which Nature smiles, and disperses in empty air!

they were the refuges of invalids on account of their peculiar salubrity.

The first great territorial change in the Roman vicinity appears to have occurred after the invasion of Attila, when the Tiber broke loose, and the Campagna became a marsh. The drainage was renewed under Theodoric; but, on the expulsion of the Goths, this tract was again neglected, and fell back into the same state. Various attempts were subsequently made on the Pontine Marshes, yet with little success. But, whatever may be the difference between ancient and modern Rome, in the degree of insalubrity, there can be no question that malaria prevailed at an early period, as well as at present. Solinus and Dyonisius inform us, that the first settlers were obliged to abandon the Palatine Mount, in consequence of the pernicious exhalations of the Velabrum; and Columella states, that the land near Tusculum, cultivated in the first Punic war, was pestilential—"the malaria of that tract being probably produced by the present Lago di Castiglione." Dr. M. thinks it probable, that the larger proportion of the pestilence described by the Roman writers, were unusually severe visitations of the marsh fever, though it is not impossible that some of them were contagious diseases. Livy informs us that, in the short period of 173 years, viz. from 287 to 460, A. C. there occurred nineteen distinct plagues, none of them at longer intervals than seventeen years, and some lasting two or three years together. This fact alone, says Dr. M. renders it impossible not to conclude, "that the *fever of malaria* must have prevailed then in as great severity as it does at present." We do not see this inference so clearly as our author. The nineteen distinct visitations, with intervals of seventeen years or so, look more like contagious diseases than malarious fevers. Why the long intervals? Why the continuance of an epidemic for two or three years? These are not the characteristics of the modern malaria of Rome. Rome is healthy in the Winter and Spring—sickly in the Summer and Autumn. But when a contagious malady enters a city, or is generated therein, we know not what may be its duration. Dr. M. does not seem to contemplate the possibility of a disease becoming contagious which had arisen from malaria; yet there are few facts in medical science better established than this. It is a phenomenon, too, which explains many things that are otherwise inexplicable.

In the time of the Republic, we have the direct testimony of many writers, as to the existence of malaria. Cato mentions places where it was impossible to live, on account of the badness of the air; and Livy speaks of tertians and quartans. Varro, with more of the Jew than the philosopher about him, advises the proprietor of an unhealthy farm to sell it at any price. The early and continued attempts at drainage render it highly probable that one object, at least, was to improve the salubrity of the soil.

Our author hazards a conjecture as to the

cause of the great difference in the extra-urban population in ancient and modern times. He remarks, that, in the same soil, and under the same degree of drainage, a tract of land under the plough is less injurious than in pasture or meadows, "whence it is possible, that the greater salubrity of ancient times was an effect of a cultivation, forced or demanded to a greater extent by the superior political condition of Rome at that time." This is extremely probable; while we may bear in mind, that the ancient Roman population was in a state of wealth and power, which would always attract multitudes from the four quarters of the globe to fill up the vacancies caused by disease. Thus, although Egypt was never without its plagues and its fevers; yet a vigorous government and an industrious people contrived to maintain, in spite of them, a condition of population and wealth which has failed only under the more exterminating malaria of Turkish ignorance and despotism. The same reasoning will apply to Venice, which is fast hastening to the same fate. It was long noted, even in modern times, for its peculiar salubrity; but "is now rapidly undergoing a depopulation, in which disease, formerly unknown or unnoticed, is taking its share."

The sixth chapter of the very interesting work under review is dedicated to the consideration of those revolutions and changes which take place with regard to the production of malaria. Many of the topics, however, have been more or less anticipated in the preceding pages. The simplest and the best known case of diminution of malaria, is that which arises from the drainage of marshes, swamps, and fens. To this measure, both governments and individuals have always had recourse—and this is the great change to which we must attribute the improvements of our own island, as well as of many other countries on the Continent. In tropical climates, little has been done in this respect.

"It is from casual reading of various kinds indeed, that we must ascertain the prevalence of fevers and intermittents during the ruder periods of our history; but when we can, by receding upwards from our own time, discover a gradually greater prevalence of such diseases, and when we find the melioration, reversely, following very accurately the progress of agricultural improvement, the whole conclusion appears to be amply justified." 181.

The local instances are innumerable. The agricultural improvements in Lincolnshire, for example, have been closely followed by a proportionate diminution of the diseases of malaria. But, in low countries, the drainage is difficult, and although the insalubrity is lessened, it is not annihilated. This is the case in Lincolnshire, Holland, and many other places. Indeed, the drainage may obviously be sufficient for the purposes of agriculture, but ineffectual for the eradication of malaria.

The overflowing of rivers reverses the scene, and gives an increase to the production of malaria. And here our author offers a very ingenious and satisfactory reason for one mode

of the said increase at the embouchures of rivers. It is this. All rivers bring down more or less materials from the mountains and countries through which they flow. These deribs or alluvia are deposited at the mouths of the said rivers, and, by raising the bed of the stream at this place, cause occasional inundations of the neighbouring grounds, and thus lead to the formation of embankments. But the bed of the river still continuing to rise, the adjacent grounds at length become considerably below the level of high water, and then drainage becomes more and more difficult, with a proportionate increase of malaria.

"Thus they tend to raise the water in its bed, and, consequently, to cause it, on any increase, to overflow, still more certainly, the lands around. And as this effect is the very consequence of the embankment, so, at any given point, the bank must be made to keep pace with the rise of the channel, that the restraint may be effectual and constant. Hence as the river becomes more elevated, the ultimate result is the same as if the surrounding lands had been depressed to the same amount: and thus, while the stream which drained them once can drain them no longer, they become, first, meadows, and ultimately marshes. And if, in the former condition, they can still be drained by means of canals and floodgates, this process becomes in time inefficient, and recourse must be had, as in Holland, to lifting machinery." 199.

There are many causes of those revolutions which take place in the salubrity or insalubrity of countries or local districts. Changes in the mutual level of the sea and land, ascribed by geology to the subsidence or elevation of the latter, as connected with the cause of earthquakes, are a fruitful source of the said revolutions. But we need not dilate on these topics. The reader anxious to gain farther information will consult the work itself.

This brings us to a natural division of the investigation, and to the middle of the volume under review. Our analysis could not be conveniently contained in one article, and, therefore, we shall defer our conclusion of it till our next number; when the propagation of malaria—the climates and seasons most favourable to its production—the geography—the nature of malaria—and, lastly, the general effects of this poison on the human constitution, will be fully considered.

We cannot close this article, however, without expressing our admiration of the industry and research by which Dr. Macculloch has collected together a most astonishing mass of information on minute points of medical topography. The manner in which the local descriptions and details are given, induced us to think, that a great proportion of them were collected on the spot by our author in person; and we were not a little surprised to learn from a gentleman, while writing these lines, that Dr. M. has travelled very little indeed. We verily believed that he had traversed half the globe in quest of the materials which he has collected, and that, in Italy, at least, (the

emporium of malaria) he had not left a foot of the Campagna di Roma unexplored. This does not derogate from his credit; and we will say, that Dr. M. has travelled round his library to some end. He has there seen more than we have seen in 20 years perambulations round this earth. In our next number, we shall present our readers with a summary of the remaining chapters in Dr. Macculloch's work, which we have no hesitation in recommending to the attentive perusal of the Profession, as a volume abounding in matters equally curious and important.

From the London Medical and Physical Journal.

ON THE PHYSIOLOGY OF THE IRIS.

By EGERTON A. JENNINGS, Member of the Royal College of Surgeons.

Physiologists, in their attempts to account for the motions of the iris which produce contraction and dilatation of the pupil, have generally adopted one of the two following theories: either they have supposed the iris to be composed of two sets of muscles, one arranged circularly, whose contractions diminish the aperture of the pupil; the other composed of straight fibres, whose contractions dilate the pupil: or else they have supposed the substance of the iris to be an erectile tissue, its dilatation or contraction depending on the quantity of blood circulating through its vessels.

There are, however, numerous facts that cannot be accounted for by either of these theories. Thus, if the former theory be true, the dilatation of the pupil caused by a large dose of digitalis, or by the local application of belladonna, and the same effect as caused by pressure on the brain and by death, can only be accounted for by supposing its muscles to be deprived of nervous influence. That being the case, the same cause that deprived the sphincter muscle of its nervous influence should also deprive the straight fibres of theirs, and the iris should remain in an inactive state, between contraction and dilatation; or it should become paralysed, in whatever state it might be at the time the cause of the paralysis was applied. But this we know is not the case,—the pupil is always dilated.

Again, when the anterior part of the iris is irritated, contraction of the pupil takes place. If there were both straight and circular muscular fibres, both being equally stimulated, the iris should remain stationary, or alternately dilate and contract, from the two muscles alternately acting, as the flexor and extensor muscles of a limb do, when the nerves leading to them are stimulated. Supposing the sphincter muscle to be much the stronger of the two, the contraction might arise from its overpowering the straight fibres; but then, when belladonna is applied to the eye, the straight muscle, being the weaker, should be first paralysed, and we should have the pupil more contracted, at least for some time before the dilatation commenced. A theory which can-

not account for such important facts as these, cannot be correct. Let us now examine the other, which supposes the movements of the iris to depend upon the injection of its vessels.

The first objection to this theory is, that we have no proof of an increased supply of blood being sent to the part at the time of its dilatation; while, in the next place, we have distinct proof that an increased quantity of blood may be circulating through the iris, and yet the pupil be preternaturally dilated. This is the case when belladonna is applied during inflammation of the iris. Here, though we see the anterior ciliary arteries injected with red blood, yet the pupil is much dilated. This it most certainly should not be, if the dilatation of the iris depended on the quantity of blood circulating through its vessels.

It may be asked, can any theory be offered capable of accounting for these facts? I think there may; and one that in a great measure removes the perplexity that has surrounded this interesting subject. But, before offering any theory, let us examine the anatomical structure of the parts in question, and endeavour from that to draw some legitimate conclusion concerning the function of the different parts.

If we examine the anterior surface of the iris, we can plainly distinguish a small portion, extending round the pupillary margin, differing in colour and appearance from the other part. This, in the best delineations of the iris, is represented as composed of circular fibres, and I am inclined, for reasons I shall hereafter assign, to believe it to be a circular muscle.* The nerves distributed to the iris arise from two sources: from the third pair, and from the first division of the fifth pair of nerves. The latter, as far as we are acquainted with its functions, as distributed to the upper part of the face and head, is entirely a nerve of sensation, while the former only presides over muscular motion.

The distribution of a muscular nerve to this organ would, of itself, be a strong presumptive evidence that some part possessing muscular power did exist, and would render it probable that the motions of the iris depended in some way on muscular action. This supposition is rendered still more probable by the fact that, in some animals, as the rabbit and guinea-pig, the third is the only nerve that sends filaments to the iris. Hence, as contraction and dilatation of the iris takes place in them, its simple motions must be radically independent of the fifth pair of nerves, however much they may influence the time and circumstances under which it acts.

Having given the reasons that principally induced me to think the motions of the iris dependent on muscular action, I shall next endeavour to point out the mode in which its different motions are effected.

* By the term muscle, I would be understood to mean an arrangement of animal fibres having a power of contractility dependent on nervous influence.

The dilatation of the iris, and consequent contraction of the pupil, I believe entirely depend upon the contraction of a sphincter muscle, situated at the pupillary edge of the iris. This I am induced to believe from the fact that all stimuli applied to the nerves of the part occasion this contraction; and any thing that tends to paralyse these nerves, as the application of belladonna, pressure on the brain, and death, all deprive this muscle of the power of action. On the other hand, I believe the contraction of the iris, and consequent dilatation of the pupil, to depend on the elasticity of the parts situated between the sphincter muscle and ciliary margin, a power distinct from muscular contraction, as it is independent of nervous influence, and even of life itself. This would account for the dilatation of the pupil when the supply of nervous influence is cut off; and it is not the only case in which nature has placed simple elasticity as an opponent to muscular contraction. In respiration, the elasticity of the cartilages of the ribs draws down the chest when the muscles of inspiration relax; and, from their elasticity continuing after the inspirators are deprived of all supply of nervous energy, expiration is the last act of life. The contraction of the iris taking place at, and continuing after, death, certainly shows that it also depends on elasticity, a power uninfluenced by the vitality of the parts.

I had recently a case under my care, showing very decidedly the influence of the ciliary nerves on the contraction of the iris. The following I extract from my notes of the case:

"J. Green, a labourer, was accidentally struck on the upper part of the eye by the lash of a carter's whip. I saw him soon after the accident, and found the upper eyelid cut. On the upper surface of the globe of the eye, about two lines from the cornea, there was a slight extravasation of blood on the sclerotica, beneath the conjunctiva. The iris presented a curious appearance: it was contracted along its upper margin, while the rest of it was dilated; the pupil was thus drawn nearly to the ciliary margin at the upper part of the eye, to an extent corresponding with the extravasation on the sclerotica. On exposing the eye to a strong light, the upper portion of the iris was fixed, though the other parts dilated and contracted as usual."

From the paralysis of the portion of the iris being so immediate, and so exactly corresponding with the extent of the injury done to the sclerotica, I think myself justified in concluding that it must have been caused by concussion of the ciliary nerves; and the iris being so closely contracted at the part corresponding to the injury, shows that its contraction is different from muscular action, being independent of nervous influence.

Mr. Lawrence has met with two cases, showing still more decidedly that the dilatation of the iris depends on muscular action, (at least on a power which, like it, owes its action to the influence of nerves of muscular motion:) he does not, however, seem to have drawn

any conclusion from them. He says, "I have seen two cases* in which there was a paralysis of all the parts supplied by the nerves of the third pair, viz. three of the recti muscles, one of the oblique muscles, and the levator palpebra superioris; so that the upper eyelid could not be elevated, and the globe was drawn outwards by the external straight muscle. In both instances the pupil was largely dilated. You may, perhaps, suspect that the optic nerve may have been insensible; but this was not the case: for, in one of those instances, when the patient looked through a minute opening in a card, producing what may be called an artificial contracted pupil, vision was perfect."

These cases are, in some measure, similar to the one I met with, as far as the iris is concerned; but they are much more satisfactory, as the paralysis was more complete; and, as the fifth pair of nerves appear to have been in a perfect state, they prevent the possibility of our attributing the contraction of the pupil to their influence.

The fact that the pupil is contracted during sleep may be thought opposed to the idea that

its contraction is dependent on the action of a sphincter muscle; for, at that time, muscular energy seems to be suspended. I do not, however, apprehend it to be any objection. The sphincter muscles differ from the straight muscles, inasmuch as they never appear to be fatigued by action. Thus, when the general muscular system is in a state of rest during sleep, the sphincter ani and the sphincter vesicæ both continue to act: so does also what I have termed the sphincter muscle of the iris. This, therefore, seems only to render it more closely allied to the other sphincter muscles.

What then, it will be asked, is the function performed by the branches of the fifth pair of nerves distributed to the iris? That they are sent there for an important purpose, no one can doubt. I have observed some facts which may, I trust, tend to throw some light on this interesting subject. But this paper is already too long to permit of my entering upon that subject at the present time. I shall, however, continue my investigations, and probably, at some future period, communicate the result of my observations.

From the London Medical Repository and Review.

CASES ILLUSTRATIVE OF THE VIRTUES OF PRUSSIC ACID, IN STOMACH COMPLAINTS.

To the Editors of the London Medical Repository.

Gentlemen,—Several years have elapsed since it fell to my lot to review† Dr. Elliotson's valuable little essay on the efficacy of this remedy in certain gastric disorders. The scanty knowledge I then had of its powers induced me to speak favourably of it; and, I rejoice to say, that subsequent experience has fully justified the opinion. In the course of my practice many instances of the remarkable powers of this medicine has occurred; but the subjoined cases are those only of which I have preserved any particulars.

Names.	Age.	Duration of Complaint.	Symptoms and History.	Duration of Medicine.	Events.
Miss Harris	36	sevl. months	{ Pain in epigastrium—vomiting urgent }	3 days	Vomiting and pain subdued.
James Sumner	30	sevl. days	{ Pain, with occasional convulsions }	7 do.	First dose greatly relieved.
Mrs. Russell	39	month	Severe dyspepsy	6 do.	Completely cured.
Sarah Ellis	25	year	Ditto	10 do.	{ Do. first dose removed eructation and flatus.
Eliz. Emery	26	do.	Ditto	5 do.	Completely cured.
Phoebe Miles	33	do.	Severe dyspepsy with pyrosis	17 do.	{ Ditto, pyrosis relieved by first dose.
William Harris	29		{ Dyspepsy with violent palpitation of the heart }	8 do.	Completely cured.
Eliz. Willis	31		Violent dyspepsy	7 do.	{ No complaint, except a little flatulence remaining.
Ann Duffin	52	week	Ditto	14 do.	{ Quite cured, very liable to this complaint.
Sarah Joyce	47		{ Gout annually for upwards of twenty years, and occurring usually in March—always preceded by evident dyspepsia, which she now has, (Feb. 20th, 1821,) together with the gouty pains in the right foot and ankle }	7 do.	{ Immediate improvement of the digestion, followed by diminution of pain in the foot. At the end of a week perfectly free from the disorder. March 27, still remains quite well.

* Lancet, vol. ix. p. 245.

† Vide Medico-Chirurg. Review for 1820.

<i>Names.</i>	<i>Age.</i>	<i>Duration of Complaint.</i>	<i>Symptoms and History.</i>	<i>Duration of Medicine.</i>	<i>Events.</i>
Thos. Thompson	24	2 months	This was the severest case of dyspepsy I ever saw, accompanied by a most alarming palpitation of the heart, and sudden and profuse eruptions of sweat. This patient had been previously and largely bled under the idea of organic disease, and taken a variety of medicines without benefit	14 days	In four days the dyspeptic symptoms and palpitation were greatly relieved. In fourteen days he was quite free from complaint—a more decisive proof of the efficacy of the remedy cannot be conceived. It had resisted every other remedial measure.
Ann Croft	33	3 months	Severe case of dyspepsy with palpitation	26 do.	Perfectly restored. Pain of epigastrium and palpitation yielded almost immediately, but the flatulence remained. She had used, alteratives and bitter tonics previously, but without benefit. This case was protracted by an accidental catarrh.
Anna Herbert	50	5 years	Distressing pyrosis and flatulence, with eructations, loss of appetite, and oppression after meals	12 do.	Perfectly restored; first dose produced remarkable alleviation.
James Wicks	30	3 months	Violent palpitation of heart; tenderness and fluttering at epigastrium. Gnawing at stomach with oppression	4 do.	V. S. and other remedies were useless. In four days, palpitation, with the other symptoms, had almost subsided.
Eliz. Silver	60	6 months	Vomiting very frequent; pyrosis; pain and fluttering at epigastrium; headache; irregular appetite; emaciation	8 do.	In two days great relief; and in eight days such a mitigation of all the symptoms, as almost to leave her entirely free from ailment.
Mary Verrey	27	2 years	Spasm at stomach with flatulence and borborygmi, producing epileptic attacks	5 do.	All the stomach ailments were suspended, and during the fifteen days, the fits did not recur.
Sarah Rickets	23	14 days	Pain in epigastrium, tending to left hypochondrium; palpitation; dyspepsy; difficult respiration	10 do.	Remarkable alleviation after the second dose. In ten days the disorder wholly subdued.
Fanny Marcham	52	4 months	Severe dyspepsy, with distressing palpitation of heart. Has used other remedies without benefit	14 do.	Palpitation gone, and all the other symptoms.

It is here requisite to state that the dose of the medicine seldom exceeded three minims. It was administered generally thrice in the day, with a little mint water, and never could I discover any inconvenience from the remedy, except a slight nausea, or certain trifling nervous sensations—both of which symptoms, however, would speedily subside on its discontinuance. Those cases of dyspepsy in which the heart becomes violently and sympathetically affected, as well as those in which the complaint induces convulsive affections, seem to be peculiarly under the control of the Prussic acid. In such instances, as, indeed, in almost all the instances in which the remedy proves efficacious, its beneficial influence is so sudden as to convince me that its operation must be primarily on the sentient extremities of the gastric nerves, thereby restoring them to their healthy function, and inducing healthy secretions as a consequence. Whether this speculation may accord with the sentiments of others, I know not; neither is it a matter of the slightest consideration, so long as the astonishing powers of the remedy continue to be supported by a multitude of facts, as clear and convincing as the nature of the subject will admit of. There are, I am aware, many stomach affections, over which it seems to possess little or no power, especially those in which the organ itself is structurally altered; but I am equally persuaded, that every practitioner who gives the Prussic acid a fair trial, will not fail to discover that he is in possession of an engine which will often enable him to overcome difficulties, insurmountable by all ordinary methods.—I am, gentlemen, your obliged servant,

F. BAILEY.

From the Medico-Chirurgical Review.

MEMOIR ON THE TREATMENT OF RHEUMATISM. By Dr. CAZENAVE, of Pau.

Pau, the town where our author resides, is only six leagues from the Pyrenees. The proximity of these mountains, and the prevailing westerly winds which blow over them, cause most abrupt and extensive transitions of temperature. The country, however, is extremely healthy, with the exception of rheumatism, which may be said to be there endemic. It simulates, like hysteria, almost every other complaint—or complicates itself with almost all other pathological conditions. In such a situation, an observant physician has excellent opportunities of studying a disease, which, after all that has been written on it, is still as obscure in its nature as it is rebellious to treatment. During a period of eight years, Dr. Cazenave has cultivated the study of rheumatism in all its ostensible and masked forms, and now comes before his brethren with a method of treatment which he considers as unusually successful.

Unlike some of our modern monographists, Dr. C. has not constructed a volume out of preceding writers:—he has merely made a few remarks, the result of his own observation. We shall extract a passage or two from this Essay, before we state the Doctor's *methodus medendi*.

"I have seen (says he) rheumatic patients suddenly seized with violent pain in some part of the body, and over a considerable space. On examining with their hands to find out the precise spot, they would have much difficulty in determining it, however hard they might press. At length, a single point, as it were, would be found to be the seat of exquisite pain and sensibility, without any thing being visible externally. In other cases the pain will suddenly cease in a part, and the patient will experience a disagreeable sensation of formication rather than distinct pain in some other part, at a considerable distance. On examination, he is surprised to find a large patch of redness, with more or less tumefaction, in the new seat of the rheumatism. In this manner the complaint will sometimes travel over the whole surface of the body, and then fix itself at the extremity of a finger for a month or two, causing dreadful pain, but without the least discoloration or swelling of the part.

"I have seen this disease in the person of a medical gentleman, fix itself in the sclerotic coat of the eyes, at their outer and inner angles, and there occasion the most terrible sufferings, on the least motion of the eye-ball. Yet the sight was not in the least affected, nor was there any appearance of inflammation. At other times the rheumatism has seated itself in the transparent cornea, attended with indescribable sufferings, intolerance of light, insomnia, and violent ophthalmia. The same medical gentleman experiences occasionally the rheumatic pain in a single point of the minutest dimensions, in the eye-brow, and other

parts of the face. An elderly lady, afflicted for several years with rheumatism of the deltoid muscle, lost slowly her sight. An ophthalmic surgeon discovered a cataract in each eye. The operation was performed on one of them; but scarcely was it finished, when the organ became the seat of the most excessive pain—the sclerotic became gorged—the coats red—and this state of insupportable sufferings lasted six weeks, with scarcely any intermissions. At this period, the deltoid muscle (which had been free from pain) became again the seat of rheumatism, and the ophthalmic inflammation and pain disappeared. She recovered sight in this eye. Three months afterwards the other eye was operated on, and again the pain and ophthalmia took place as violent as in the other eye. A blister was applied to the arm, and a metastasis of pain was quickly produced, and the eye relieved."

Of all the INTERNAL organs, our author has found the STOMACH the most liable to rheumatic affection. In some cases, this shows itself merely in languor of function, or simply a sense of cold or pain in the epigastric region, relieved by hot frictions. The digestion may be very little impeded. In this state of chronicity, the disease is difficult of removal. But, not unfrequently, it produces in the same organ much more disagreeable effects, as nausea, vomitings, indigestion, violent cardialgia, and symptoms imitating cancer or scirrhus of the pylorus—all which phenomena, will suddenly disappear on the commencement of rheumatic pain in some of the limbs.

"I have seen a case of wandering rheumatism, where, after attacking the stomach, the bowels, &c. it fixed itself for more than three months, on the heart, inducing palpitations, convulsions, syncope, and other symptoms that led the attendant physician to believe there was aneurism of the heart. A blister applied to the arm dissipated the whole of these symptoms, and the patient afterwards enjoyed good health."

The author has seen the bladder affected with rheumatism, and retention of urine produced—the lungs attacked, and all the phenomena of peripneumony succeed. Parturient women are very susceptible of the causes of rheumatism, and Dr. C. avers that nothing is more common, at least in his part of the country, than rheumatic pains in the uterus and its appendices.

But we must now proceed at once to the treatment which Dr. C. has brought forward, as it is somewhat novel—at least it is a new modification of a remedy which has long been employed, though less so in this country than formerly. It is OPIUM. After remarking on the different effects of opium, according to the dose, or the repetitions of the doses, Dr. Cazenave proceeds to maintain, that the failure of opium in the cure of rheumatism is owing to the timidity with which it is administered. In the complaint under consideration, Dr. C. remarks, opium acts in three ways, according to the dose employed. Given in small quantities, it obtunds the sensibility, and brings a

temporary relief—but the cure is not thereby accelerated. Administered in a somewhat larger dose, it sometimes occasions nausea, palpitations, giddiness, headach, &c. These effects are, of course, but momentary, and should form no solid objection to the remedy, if it is found beneficial in other respects, besides relieving pain. To the above effects of opium (if it be continued) succeed others:—the patient does not sleep; but he experiences a kind of delightful ecstasy, forgets his sufferings, &c. The action of opium is then excitant, like that of wine. In some cases, an abundant perspiration is the result—but, in both events, the radical cure of the rheumatism is effected—that is, with or without the sweating process. The quantity of opium will vary, of course, in different constitutions; but the following is the mode of administration employed by our author.

To an adult, he orders a pill containing one grain of opium—and, an hour afterwards, he gives another grain, if the pains continue. At the expiration of the second hour he gives a third grain—and, after a little time, he examines his patient. If there be a tendency to hilarity, he administers a fourth grain, and so on, a grain every hour, till a complete calm is established, or an abundant perspiration is induced. This being the case, he orders a grain to be given every two, three, or four hours, according to circumstances, solely with the view of keeping up the perspiration.

In respect to regimen during this mode of treatment, it is indispensable, of course, to keep the patient in an even and mild temperature, with flannel next the skin, and on the simplest liquid food. Perfect quietude is necessary during this treatment. In this way, Dr. C. assures us that he speedily cures rheumatism, whether acute or chronic, or in whatever part of the body it may be seated, without any bad consequences ever ensuing. When the disease is complicated with any other complaint, particularly with derangement of the digestive organs, it will be necessary to attend to the adventitious disorder. If the fever in acute rheumatism run very high—and particularly if any thoracic or abdominal organ be oppressed in function, or labouring under pain, it will be proper to draw blood from the general system, and to put in force the other items of the antiphlogistic treatment.

We think the plan of Dr. Cazenave is not unworthy of attention, in the management of a disease which so often baffles the medical practitioner, and brings no small degree of odium on his art.

From the Edinburgh Medical and Surgical Journal.

CASE OF LABORIOUS PARTURITION,
complicated with Laceration of the Uterus and Vagina. By WILLIAM CAMPBELL, M. D., Lecturer on the Practice of Physic and Midwifery, &c.

Mrs. Ferguson, aged 36, of rather spare habit, the mother of seven children, was seized

with uterine action on the 21st of November 1824, at 9 P. M., while in the early part of the last month of this her eighth pregnancy. Pains continued until between one and two in the morning of the 22d, when they suddenly, almost entirely, ceased. After the cessation of uterine action, the pulse became extremely rapid, small, and indistinct, accompanied by a trifling effusion of blood *per vaginam*.

The patient was under the care of my friend Mr. James Scott of Broughton Street; and I saw her about an hour after the character of the pains had become changed. She was then in a most alarming state. The abdomen could not bear the slightest pressure without violent pain being produced; the pulse was too quick and indistinct to be numbered; the countenance pale and collapsed; considerable thirst was complained of; and there was a disposition to bear down, but without effect. Immediate delivery was resolved upon, and, to accomplish this, forceps were applied over the head, which was placed upon the brim, but from its great size, it could not, after a cautious trial of half an hour, be brought through. The forceps had nearly slipped off the head during the attempts of extraction, from the elasticity of its bones, whereby we were induced to think that the cranium contained water. The forceps was withdrawn, and the perforator pushed into the anterior fontanelle, which gave exit to a considerable discharge of water; and this was soon followed by the expulsion of the fœtus, and about 2 lbs. of coagulated blood. As the uterus was in a state of relaxation, and blood continued to be effused *per vaginam*, the hand was introduced into the passages, when an extensive rent was discovered in the upper and back part of the vagina, extending through the cervix to the lower part of the body of the uterus. The hand passed through this opening, among the intestines, before we were scarcely aware of the accident. The pelvis was well formed; and the patient had an easy time in all her previous labours.

At half-past one P. M. vomiting of dark coffee-coloured matter commenced, and the quantity brought up in the course of the day amounted to about 2 lbs. While in this condition Professor Duncan, Jun. saw the patient, and agreed with Mr. Scott and myself in thinking her in a most unfavourable state. The pulse ranged from 130 to 150 in a minute, and the lower extremities occasionally felt cold. Beef-tea and port wine, in small quantities, were allowed for support, and bottles of hot water were applied to the lower limbs. The patient had four enemata in the course of the day, each containing a pound of beef-tea and ten drops of the tincture of opium. 23d, 10 A. M. Pulse much improved in firmness and regularity, and beat 126. There were occasional pains in the abdomen; lochia moderate; urine to the amount of six ounces was drawn off last night by the catheter. 6 P. M. Black vomit reappeared, and continued until about six pounds of it were brought up. The bladder was evacuated several times in the course of the day with ease. The passages were

washed out with about twelve ounces of warm water; and the following draught was ordered at bed-time.—*R. Solut. opii sedativ. gr. xj. aq. menth. ℥j. M.*

24th.—Pulse as yesterday; no vomiting since last night, though a considerable quantity of beef-tea has been taken by the mouth. The patient had a very fetid alvine evacuation this morning, and she complains of considerable pain and tension of the abdomen. At this visit she was ordered a pound of gruel and two ounces of the volatile oil of turpentine for an enema; to have the abdomen fomented by flannel compresses wrung out of one pound of warm water and four ounces volatile oil of turpentine. 6 P. M. Enema procured three copious evacuations of feces and flatus, which were followed by great relief. An examination was made *per vaginam* at this visit, and the situation of the rent in the posterior part of the *cervix uteri*, extending towards the promontory of the sacrum, could be traced as far as the fingers could be advanced. There was no visceral protrusion, and the uterus did not seem at all sensible, as the patient scarcely complained of any uneasiness during the examination.—*Haust. ut heri, nocti hora somni habeat.*

Nov. 25th.—Patient had a good night; pulse 114, firm and regular; tongue and skin moist; no disposition to vomit for the last two days; tension of the abdomen less; had an alvine dejection in the morning. Beef-tea and wine were ordered to be discontinued, and a little panado substituted.—*Haust. ut heri nocte rept.*

26th.—Stools have been thin and frequent, accompanied with a considerable degree of tormina and tenesmus since the exhibition of the turpentine enema.—*Habeat statim ol ricini ℥ss.*

She was ordered a little rice boiled in milk, for nourishment.

27th.—Pulse 112; skin temperate; diarrhoea going on.—*R. confect. aromat. ℥ij. tinc. kino ℥j. tinc. opii, gr. xl; Tere bene simul, et postea gradatim adde, aq. cinnam. et aq. pur. ana ℥ijss. Fiat haustus, cujus ægra cochleare amplum, quæque tertiæ hora sumat.*

To be allowed a little chicken for nourishment.

29th.—Pulse 102; mammae a little tense and painful. Patient continued to regain strength, and we gave up visiting her on December 2d. She remained long delicate, and has not conceived since. During the whole of this pregnancy her feelings were very different to what they had been on former occasions; she felt herself weakly, heavier, and not so active as in her former pregnancies.

The fœtus, independent of its being rather small and hydrocephalic, had *spina bifida* on the lumbar portion of the spine, and its lower extremities were much distorted. The cranium of this fœtus, which is now among my collection, is to a great extent membranous. Its circumference from the chin to the vertex is 11 inches; from the chin to central portion of the sagittal suture 8½ inches; from the root

of the nose to the vertex 9½ inches; and from one parietal protuberance to the other 8½ inches.

4, Picardy Place, Feb. 27, 1828.

From the Edinburgh Medical and Surgical Journal.

CASE OF DIFFUSE INFLAMMATION OF THE CELLULAR MEMBRANE from a Poisoned Wound, terminating favourably under the use of deep incisions.
By JOHN EVANS, M. D., Member of the Royal College of Surgeons of London.

Mr. Layrd, surgeon of the Malin Dispensary, aged 26 years, living in a healthy part of country, surrounded by sea air, exercising freely, of regular habits, and in other respects possessing a good constitution, was called, on the 19th May, 1827, to open an abscess in the scrotum of a poor man advanced in life, which was occasioned by retention of urine, whereby the urethra sloughed, and the urine was infiltrated into the scrotum and penis.

To give exit to this fluid, Mr. Layrd made many punctures into the distended parts; and in making one of these, his right thumb, smeared with this sloughing matter, was pricked with a pin below the nail in the act of turning up the sleeve of the coat.

Acute pain, and more than what commonly attends a similar injury, was then experienced. The part was forthwith washed and well sucked. The person who gave rise to the accident died the following day. Mr. Layrd now went to a neighbouring lake, and fished for some hours. He returned to dine at six, and in taking hold of a tumbler of warm liquor in the wounded hand, he, for the first time, felt an alarming sensation of pain and heat so pungent, that he almost dropped the glass on the table.

From this moment a train of severe complaints commenced; nor can language sufficiently describe the sufferings of the unhappy individual. Up to the time of my first seeing Mr. Layrd, I had the history and treatment of the affection, as well from himself as from Mr. M'Dermott of Carn, who was most assiduous in his attendance on Mr. Layrd.

It appears the pain, uneasiness, and burning sensation were confined to one spot, and this where the pin had entered, not being larger in extent than what a pea would cover, now became insupportable. He ran from place to place, scarified it and applied lunar caustic. Then he immersed the hand in warm water, when it became worse. This was changed for cold, but without alleviation. A warm poultice was next applied; but the pain increasing, this was laid aside, and adhesive plaster put on. Thus the night was spent in the most miserable way, without relief from any application.

20th. He was no better. A crucial incision was made over the thumb, and goulard put on. He was bled to sixteen ounces; but becoming weak when this quantity was abstracted, the vein was closed. Three grains of calomel, followed by sulphate of magnesia, produced five dejections.

P. M. 8th hour. I saw him for the first time twenty-six hours after the commencement of symptoms, and thirty-seven from the period of receiving the supposed poison. The pain indeed was now extreme. He had derived no benefit from the bleeding. Pulse 96, certainly one from its compressibility not demanding further depletion. He was in bed, tossing to and fro; his face flushed, as well from muscular exertion as from the warmth of the night and a confined room, from febrile reaction. It was distressing to see him gnashing his teeth, beating the bed with his feet, and calling out with the fiery sensation in his thumb. The blood taken was natural; nor was there any increased heat of body. Tongue dry, and brown in the centre, like that of a typhoid patient on the third day of illness. The thumb now was red, swelled and painful; the burning sensation, which was constantly present, aggravated on the slightest motion or pressure of the part. He dreads approaching it. These inflammatory indications, with high morbid sensibility, are confined to the first joint of the thumb.

On examination, the lymphatics are seen running up the arm in two distinct lines on the inner side, to within three fingers' breadth of the axilla. The hand is now in cold water, often renewed; nor can he keep it one moment out of it. Three large leeches, all we had, were put on the wounded part. His calls were most urgent for the cold water during their application. The extracts of *Belladonna* and *Hyoscyamus* in watery solution were freely applied. On the sound skin, above the termination of the inflamed lymphatics, an eschar was formed circularly, by pencilling the part with nitrate of silver. He was ordered five grains of compound powder of ipecacuan, and one grain of sulphate of quinine, every second hour, with cold drinks.

3d day of illness.—The Dover's powder was reduced to half the quantity, the third dose causing retching. Some sleep, followed by frightful starting. Gently pressing the wound caused dark fluid blood to come away. The inside of thumb distended; the integuments tense; pain and uneasiness unabated. This part was now freely incised, till the knife reached the bone. Every thing of yesterday continued. Urine voided in large quantity and clear. The inflamed state of the lymphatics stationary. He requested to have a trial made of warm water; but his sufferings were thereby increased, and the cold was resumed. Leeches were ordered from Derry, a distance of twenty miles; and, in the meantime, fit rollers were applied to the finger and arm. These were moistened by the narcotic solution spoken of above, and with as much pressure as could be borne. The parts were subjected to the pressure of rollers for two hours and-a-half. During this period, decided relief followed; but the leeches having arrived, the bandage was taken off, and twenty-two applied over the head and fore-arm with effect.

No relief followed this application, while the ease experienced during the pressure was

succeeded by the former pain. Incisions extended; two others made on the thumb. Pulse 90. The pain before making the last incisions was truly frightful. He exclaimed in agony of the burning sensation in his thumb. The grumous blood continues to flow; no pus from any part; no motion. P. M.—Swelling decidedly advancing; it is now as high as the wrist.

Pil. Hyd. gr. vi. mane. Sul. Mag. et Infus. Sen. Pulv. Dover. et Sul. Quin. h. h.

4th day.—A better night. Changed the water often from hot to cold. The inflammation extending, a blush of redness is seen in the axilla; then the skin seems natural, while beyond this a second blush is seen on the side. Tongue dark and dry in the centre; pulse 83. Grumous blood still oozing from the wound. The parts incised are less tense and painful. No shivering, headach, or unusual weakness. Intellect sound. Another incision on thumb. Gruel freely taken. Eleven leeches applied. Four natural dejections. After the leeches the pain became most excruciating. Dover's powder and quinine continued.

A glass of port negus, with twenty drops of laudanum. Nourishment in abundance. A severe rigour in the evening. Extremities cold. Laudanum and negus repeated. Warmth applied.

H. S. Tinct. Opii. gutt. xl. Mist. camp. ℥ss.

5th day.—Two hours sleep. Symptoms aggravated. Pulse 96. Another incision on the back of the wrist. Lymphatics less red. He resumed the hot water. Took three ounces of wine at two times. No motion. Tongue as yesterday. H. S.—Has taken nourishment freely. Every thing continued. *Tinct. Opii. gutt. lxxx.*

6th day.—Slept on the whole well; less pain; pulse 108. Epsom salts with effect. Two grains of quinine every second hour. No suppuration. The cuticle separated and removed from the thumb. The punctured part looks like half-boiled veal. The fore-arm is much distended; and the pain in the evening intolerable. This part was freely laid open for five inches in length, dividing the tense fascia as it presented itself to the knife.

This incision was followed by great retraction of the integuments, and ease that cannot be described; but the pain next settled on the soft parts, covering the metacarpal bone of the fore finger. This was forthwith divided with the usual relief. The finger could be insinuated under the divided edges of the integuments, and these readily everted. It seemed as if they had lost their adhesive connexion. The countenance and features look sharp and haggard. He is restless and peevish; urine clear and voided in large quantity. Every thing ordered taken. Nourishment freely, consisting of mutton-chop, soups, &c. &c. He lies now with his arm and shoulder in hot water. He is propped by pillows half out of bed; and the whole extremity, up to the shoulder, is in a bullock's feeding-trough.

P. M.—Pulse 120. No improvement in the tongue. Severe paroxysms of pain run-

ning in the direction of the radial artery. At 6 Dr. Rogan arrived, and a consultation was held with this gentleman and Mr. Irvine, R. N. The result was to follow up the practice of incision as heretofore; and whenever pain and tension become urgent, then to use the knife without reserve.

About this period a singular nervous sensation manifested itself. It was like deep and weighty sighing, referred to the præcordia, which no effort of his could overcome or restrain. The inspirations were short and with a loud noise, while the expirations were long and anxious. Two ounces of port. Laudanum repeated to eighty drops. It was applied also to the pit of stomach. The former remedies continued.

7th day.—Some sleep in the night, and two hours' freedom from pain after laudanum. After an interval of six hours it was repeated to seventy drops. The pulse and tongue as last reported. Affection of præcordia distressing. An old incision extended. At consultation it was resolved to give laudanum steadily, seventy drops every sixth hour. If the pain remains unmitigated the interval to be four hours. Sherry, one pint in twenty-four hours. Nourishment to be continued largely, paying attention to the stomach.

Laudanum taken at 10 and at 4 P. M. Wandering in intellect; inflammation extending. At 8 the laudanum omitted, and the following substituted by Mr. Irvine.—*R Ext. Hyoscyam. n. gr. viij. Pulv. camphor gr. iijss. formâ boli. Repetatur t. opii gutt. xl.*

Two incisions made over the radius and ulna were followed by immediate ease.

8th day.—Hot applications continued. He had an hour of refreshing sleep, and awoke covered with perspiration; urine copious; the inflammation extending; pulse 106. Three natural fluid dejections. Wandering of mind continues, but is collected when spoken to. He still has deep inspirations, with soreness in the epigastric region; and feels as if there was spasm in the cardiac portion of stomach. The skin is natural. Quinine continued; laudanum at the hour of rest.

9th day.—Every symptom aggravated; bowels often moved; pulse 120. He complains of pain in the direction of the œsophagus going into the stomach; difficulty in swallowing. He has taken 160 drops of laudanum in three doses. Camphorated liniment to the throat and neck.

Yesterday he was resigned and courted death; at present he is more cheerful and anxious to live. The medicines were continued. P. M. Nourishment abundantly taken; six fluid dejections. Eight ounces of Madeira consumed with malt liquor occasionally. A former incision deepened from a sense of fluctuation, was followed by blood only. He appears rapidly sinking, as well from disease as from diarrhœa. *R acetat. plumb. gr. vij. pulv. opii, gr. iv. in pil. h. s. iv. unam 3tia hora.* An enema of starch with 100 drops of laudanum. This to be repeated in four hours. Omitting the laudanum; the quinine continued.

10th and 11th days.—Pulse 106-20; inflammation above the elbow. The laudanum in full doses at night. Sleep. A bottle of Madeira daily, with as much malt liquor. The arm is now frightful; phlyctenæ scattered over it, and a large one on the fore finger; urine still clear and voided in large quantity.

12th day.—Inflammation over the shoulder. The hot water discontinued, and a flax-seed poultice applied. Diarrhœa has ceased.

13th day.—The quinine was continued steadily at the rate of two grains every second hour till this morning, when he refused every thing in the shape of medicine. The diet, consisting of the most nutritious articles, given often, and retained in large quantity, with it appears *nearly* two bottles of old Madeira in the last twenty-four hours, independent of London porter or ale, which certainly exceeded another bottle, has been duly persevered in. The wine is now restricted to half the quantity, with some ale. Pulse 116; the tongue as unfavourable as it can be. Marks of inflammation are now seen on the side; it is of a bright scarlet colour, but the tension seems less; some discharge of a puriform nature from original wound. P. M. Quinine resumed in grain doses every second hour, and the laudanum at night. The tongue moist at the edges.

18th day.—Discharge purulent from thumb, sparingly so from one of the incisions; the extremity, enormously swelled, is in girth nearly equal to a moderately sized pillow. The redness is now becoming pale; fluctuation evident through the whole arm, but the boggy feel only at the inner margin of the deltoid.

An incision was made near the origin of the *supinator radii longus*, and a large quantity of pus came off, by no means unhealthy. Another opening was made along the lower portion of the *flexor carpi ulnaris* with the like result. It was opened after this by Mr. Irvine at the part reported boggy, and matter evacuated. The greatest relief followed these several incisions. The tension became less; the arm more manageable, and diminished in size. Before this, moving or changing its position, from its weight and pain, was a task of the greatest difficulty. Tongue assuming a natural appearance. Every thing continued. Wine reduced to half a bottle in the day. Pulse 104.

19th day.—The openings continue to discharge; no sloughing of cellular membrane; the exhaustion and emaciation great. He has had severe pain in the opposite side, accompanied with much difficulty of breathing, amounting to a kind of paroxysm, and seeming to affect him so violently that he could not survive it. This continued for two hours. I saw him when at its height. Hot bricks were applied with relief. In the originally affected side, at the inferior angle of the scapula, pain and uneasiness were complained of. It was examined, and matter was discovered on the inner edge of the *latissimus dorsi*. Here again the boggy feel was well recognised. Below this abscess the integuments seemed to present a contracted or adhesive feel, defining its lower

portion. We found it did not extend beyond this subsequently. A free incision was made into this part, and sixteen ounces of matter taken away. It was left discharging.

In this matter, were seen large portions of detached cellular membrane, and clots of dark blood. Wine, diet, and quinine continued. Laudanum at night.

25th day.—Continues to improve.

July 12th, 55th day.—Every thing favourable. He was out of doors a few minutes for the first time to-day. The original wound still discharging purulent matter; that from the side is of a serous nature. No power of moving or using the extremity. He is still very much emaciated.

It is superfluous to refer to the many valuable cases published on similar affections. That now detailed justifies the opinion that neither any bad habit of body, or unwholesome condition of atmosphere, or the injury itself, could have availed any thing in inducing a disease so severe; and that all was caused by the minutest portion of an animal poison entering the system, and this poison generated during the life of the individual who communicated it. Similar occurrences do not often happen. Let the nature of the poison be what it may, it derived its virulence from extravasated urine, causing gangrene of the parts in contact with it, and involving extensively the cellular membrane. That the person imparting this poison was free from any disease till attacked with the retention of urine, may be inferred from his general healthy condition and his country life.

As caustic was early applied, and the part also early scarified, we are told a portion of it cut out, we have no knowledge whether a pustule or vesicle had formed. But the inflammation being continuous, the numerous *phlyctenæ* on the arm and fore-arm, and the absence of glandular inflammation, prove the severity of the disease, and the danger of the patient, while they strengthen the belief of an animal poison giving rise to the whole train of symptoms. We need hardly confess our obligations to Mr. Travers for his judicious remarks on constitutional irritation. Nobody will doubt there was in Mr. Layrd's case what this gentleman calls "prostration with excitement." The sighing, sinking, the indescribable uneasiness about the præcordia; the affection of breathing, with uneasy sensations in the diaphragm on the sixth day; the difficulty of swallowing; the jactitation; the state of the pulse and tongue, and the thirst, up to the formation of pus, and its discharge, all tend to establish this belief.

Throughout the course of the affection the urine was clear, and was noted by the attendants to be unusually abundant. The absence of morbid heat is equally strange. With respect to the treatment we have only to state, that without the knife, used both freely and extensively, every effort to save life would have been unavailing.

The incisions were made, in the first place, to relieve the insupportable tension and distension of the fascia, and the soft parts exterior

to this, thereby freeing the sufferer from the pain attending thereon, while the amount of pain induced him to acquiesce in each operation, as ease in every instance followed. It is the opinion of his medical attendants that these states of pain, tension, and distension, would, by their acting on the nervous and cerebral systems, have destroyed him long ere matter could have formed. But the incisions were resorted to, in the second place, to give exit to accumulated collections of matter; as in no case, save at the thumb, where the poison was first introduced, did it find a passage by the first formed ones. The cellular membrane presented a whitish appearance at each incision; afterwards it looked like rotten tow.

Thus it seemed the superficial fascia, with the soft parts exterior to this, was in the beginning the seat of the affection; while the formation of pus was confined to that covering the muscles.

Those who advocate the depleting system in the case before us, will be disappointed when they perceive that neither the general or local abstraction of blood availed any thing. I do not say that the former rendered the disorder more untractable, yet I confess my doubts of its having been of use; while the latter certainly neither mitigated pain nor assisted in arresting the progress of the malady; and it is the conviction of the sufferer himself, that in both applications the leeches did harm.

I am of opinion that the constitution was affected, first, through the agency of the nerves; then we traced the poison to the lymphatic system; and lastly, continuous inflammation, the "specific inflammation peculiar to poison," the "erythematic" of Mr Travers, pervading the cellular membrane and blood-vessels.

The grumous blood coming from the wound for so long a time appears to have been the effect of the poison, acting on the vessels locally. The inward nervous sensations, as sighing, sinking, &c. were in all probability caused by this poison acting on these systems circulating through the blood-vessels. Throughout the course of ailment the glands in the axilla remained unaffected. We need hardly say that the applications to the extremity were changed according to the feelings of the patient; while the cordial and nutritive mode of treatment was persevered in to support the system, struggling with the effects of a malignant poison.

October 13th.—Mr. Layrd has not recovered the use of the extremity. He is unable to perform almost any of the common duties with it. His health I believe to be otherwise good.

From the Medico-Chirurgical Review.

INFLUENCE OF THE STOMACH ON THE BRAIN.

In a former paper we gave some account of a dissertation by M. Bayle, on the influence of chronic inflammation of the stomach and bowels in the production of mental alienation. In

a second memoir, he adduces further examples of this influence, and winds up with certain conclusions or reflections, which we shall briefly notice.

1mo. In a few cases, chronic phlegmasia of the mucous membrane of the stomach and bowels, is capable of inducing mental alienation—and, in *many cases*, it is capable of keeping up the mental malady, and modifying its character.

2ndo. Most of those who become deranged at the close of chronic gastro-enteritis, have inherited a constitutional predisposition to the disease from their parents. Their mental faculties are generally weak—the brain irritable. The dyspeptic and nervous sufferings of people labouring under gastric and intestinal irritation, tend very much to mental hallucination.

3tio. The anatomical characters of the gastro-enteritis which accompanies or causes mental alienation, does not differ from that which is unaccompanied by any mental aberration.

4to. The manner in which the gastro-enteric affection acts on the head is two-fold. When the pain of the abdominal malady is great, it is sympathetically propagated to the brain, and there disturbs its functions, in the same way as it is propagated to the heart, and produces the phenomena of fever. Hence the symptoms of mania. But when the gastro-enteritis is chronic, and less intense, or the cerebral system of the patient less disposed to irritation, then the dyspeptic sufferings of the individual gradually dispose to hypochondriacism—and hence arise various kinds of monomania—and especially the dread of being poisoned, with obstinate refusal of food, &c. These two forms differ only in degree, and they often pass from one into the other.

5to. This dread of poison and obstinate refusal of food are regarded by M. Bayle as the most constant and essential symptoms of mental alienation accompanied by chronic inflammation of the mucous membrane of the digestive tube. The two symptoms above-mentioned are “the expressions of gastric and intestinal sufferings in mental alienation.” They may exist, he observes, without this condition of the stomach or bowels, but he has never seen a case where they were present, and where the symptoms of chronic gastro-enteritis did not show themselves during life, or the traces of it were found after death.

6to. It follows from these premises, if they are correct, that the treatment of insanity, where these phenomena obtain, must be specially directed to the gastro-intestinal affection; and does not differ from that which is necessary or proper where no cerebral disturbance is complicated with the original malady. This treatment hinges almost entirely on repeated leechings of the epigastrium; mucilaginous drinks; extremely abstemious regimen; exercise in the open air; counter-irritation.—REVUE MEDICALE.

Fanciful as the above doctrine may appear to those who look no farther than the organ *apparently* most affected, for the source of a disease, we believe there is some foundation

for it in fact. The mental miseries resulting from gastro-intestinal irritation are but very imperfectly known even to the most experienced physicians. Nothing, in short, but actual personal sufferings can teach the terrible but instructive lesson!

From the Medico-Chirurgical Review.

DR. KELLIE ON TUBERCLES, AND THEIR EFFECTS ON DIFFERENT STRUCTURES.

(Dr. Monro's Work on the Brain, Vol. I.)

Dr. Kellie observes, that common tubercles may be formed—attain a considerable size—and long exist in any of the various structures of the body, without producing much trouble or sensible disturbance of function. But, when they become excited, inflamed, or suppurated, then they begin their work of disorganization in the contiguous parts, and produce trains of symptoms—more characteristic, however, of the injured function of the matrix of the tubercles, than indicative of the existence of the parasitic formations themselves.

“I have found tubercles in the serous membranes, and imbedded in the substance of the liver, of the spleen, of the kidney, of the uterus, of the lungs, of the brain, and of the cerebellum, in subjects in which no symptoms had indicated their existence during life. In young people, in children especially, from five to ten or twelve years of age, it is not uncommon to find tubercles of various sizes, and in various states of progression, co-existing in all or most of the organs in the same subject, who had enjoyed perhaps good, or very tolerable health, till within a very few days of his death. I found them in the brain, in the lungs, and in the peritoneum of one boy of seven years of age; in the brain, in the medulla oblongata, in the liver, in the spleen, and in the lungs of another boy at the age of nine; and yet these boys were active and well, and continued at school till within fourteen days of their death. Both died of Hydrocephalus acutus, passing, in the course of fourteen days, through all the stages of that disease, with nosographic regularity. In the latter boy, there occurred, indeed, during the last two days of his existence, a paralysis of the right side, and a spastic rigidity of the left arm, which enabled me to conjecture the probable existence, not of tubercles, but of inflammation and softening of the substance of the brain, as well as of the distention of the ventricles by serous effusion. Accordingly, besides the dropsy of the ventricles, I found two tubercles, each of the size of a garden pea, hanging pendulous from the tentorium, two others of the same size in the medullary substance of the right hemisphere, and one oblong tubercle, of the size of an almond, suppurated in its centre, and imbedded in the pons varolii.”

This fine boy, till attacked with headach, fever, &c. about a fortnight before his death, was apparently free from disease—was active,

diligent, and clever at his school! The following case is interesting.

"On the morning of Thursday, the 2d of August, about half-past eight o'clock, I was requested to visit, as speedily as possible, a boy of the name of Bell, residing in Poplar Lane, who had a little before been suddenly attacked with violent convulsions. When I arrived, the convulsive struggle was over, but there was still an occasional subsultus of the muscles of the face and of the arms. He was insensible and comatose; the pulse was very rapid, and the heart was striking against the side with remarkable violence. My inquiries were answered to the following effect: that his age was six years; that he had been upon the whole healthy, never had a convulsive fit before, was not liable to headaches, though he was to coughs ever since he had had measles; that he was in perfect health, in so far as could be discovered, on Tuesday; that he had taken his food that day, and had amused and occupied himself as usual, going to bed at the usual time without complaint; that, on Wednesday morning, when dressing, he complained for the first time of pain of the loins, and of that only; that, continuing to do so, and seeming listless and unwell, his aunt had, towards noon, given him a dose of Epsom salts, which operated four or five times, but without relieving him; that, on the contrary, though he kept out of bed all day, he complained more and more of the pain in the back and loins till night; that, towards eleven, he fell asleep, but frequently awoke and complained of this pain, and at length, towards three this morning, he seemed to suffer so much, that Mrs. Nichol, a midwife, residing in the same quarter, and an intimate of the family, was called to assist him; that she administered an enema, and observing what she called a great working about the breast, she applied two leeches to the sternum; that the excruciating pain of the back continued unabated; that he complained incessantly, and was extremely restless till eight o'clock this morning, when the convulsive attack supervened.

"Six ounces of blood were instantly taken from the arm; another enema was then administered, and a purgative powder, was directed to be given in a spoonful of gruel, so soon as the patient recovered the power of swallowing.

"But he never recovered his sensibility. The spasmodic twitchings of the muscles of the face and arms continued to recur at short intervals; and at half-past noon there came on another general convulsion, which continued, with little abatement, till one o'clock, when he died.

"Next morning the body was examined. The general appearance of the corpse was that of a well-grown boy of his age. The medulla spinalis, to which my attention was first directed, on account of the excruciating pain complained of in the back and loins, very carefully examined in its whole length, exhibited no trace of disease in its substance,

its membranes, or its vascular system. The spinal nerves were equally sound.

"In the cerebrum I found three tubercles, and in the cerebellum one. Of those discovered in the brain, one the size of a small chesnut was found adherent to the arachnoid of the dura mater, and buried in the substance of the convexity of the posterior lobe of the left hemisphere, from which it was withdrawn along with the dura mater, on raising that membrane in the usual way from the cerebrum.

"When the softened cerebral substance, torn from the brain by the removal of this tubercle, was washed from it, the tubercle itself, of the size and shape nearly of a chesnut, appeared somewhat nodulated on its surface: it was very firm, of a grayish colour, and invested by a membranous cyst, having evident vascular and membranous connexions with the arachnoid, and with the brain. Other two tubercles were found, one in each hemisphere of the brain, imbedded in the medullary walls of the lateral ventricles in corresponding situations, a little above and between the corpus striatum and thalamus of either side."

Dr. Kellie has not met with any other case, in which a fatal crisis has so suddenly occurred at so early an age. In young people, the more common termination of such cases is by consequent arachnitis and hydrocephalus—in adults, by inflammation and softening of the cerebral substance.

From the *Lancet*.

ON PIAN, YAWS, OR FRAMBÆSIA.*

By M. FERRIER.

Pian, yaws, or frambæsia, is a disease endemical in some parts of Africa, America, and the East Indies, and is known in Canada as the Canadian disease; in Scotland, as sibbens; and in Norway, as radsyge. It is a pustular disease, which attacks all ages, all conditions of life, and as frequently one sex as the other; it is seated in the tissue of the skin, in the vessels and lymphatic glands.

This disease principally attacks the parts near the organs of generation, although it shows itself also on all parts of the body; care must be taken not to confound it with syphilis, dartres, leprosy, or other similar diseases; it differs essentially from these affections in its causes and symptoms, on which account it may easily be distinguished from them.

The predisposing causes are—a lymphatic temperament; extreme sensibility of the skin; exposure to the rays of the sun; habitual use of vegetable food; want of cleanliness; frequent use of spirituous liquors; suppression of respiration, occasioned by the sudden passage from heat to cold; remains of some diseases, such as the itch, dartres, or syphilis.

Exciting causes.—The causes which contribute to the formation of this disease, are the bad treatment to which the blacks are subjected; the nature of the ships which transport them from Africa to different parts for

* *Repertoire d'Anatomie*. Tom. 4, p. 4.

sale; the moisture of their huts, which are situated near marshy places; the custom which prevails amongst them of lending their clothes to each other; coitus, &c. This disease may also be produced by the bites of flies; but among the determining causes of pian, ought not the impression which is produced on the skin by the effluvia of marshes, and an atmosphere saturated with deleterious exhalations, to be ranked?

If it be admitted that the skin is a sponge, which imbibes humidity from the atmosphere, why should it be insensible to the miasmata which this humidity contains? I do not coincide with those who attribute, in a great degree, the development of the pianic eruption to the custom which is prevalent among the blacks of anointing their skin with fatty substances; this custom, far from being pernicious, may, on the contrary, be useful in certain countries, where the habitations are surrounded by marshes from which, in excessive heat, unhealthy exhalations, or miasmata, proceed.

Symptoms, progress, duration, and prognosis.—This disease is generally preceded by sense of weight, weakness, and pain in the joints; hard and frequent pulse, red tongue, together with great thirst. Sometimes nausea, and even vomitings, take place after meals; the shiverings, which sometimes happen, are generally followed by heat in the skin, and copious perspiration; soon after there appears, on every part of the body, particularly on the margin of the anus, in the groins, under the axillæ, &c., small blotches, or tubercles, which are violet-coloured in whites, and of an ash colour in blacks, and not so large at the commencement as the grains of pearl barley; they vary, however, in size and form, and present the consistence and hardness of warts.

The disease is gradual in its progress, the blotches are raised above the level of the skin; they vary in size from that of the head of a pin, to a large strawberry; their surface presents the appearance of the raspberry (from which the name *frambæsia* has been given by many authors, to this affection.) It sometimes happens that these pianic tubercles are so thick, that they adhere to each other; the epidermis, which covers them, is distended; it breaks, and there escapes from its surface a white, ichorous and glutinous liquid, which spreads an infectious odour, occasions great pain on those parts with which it comes in contact, whitens the hair, and causes it to fall off. This liquid is so acrid, that it acts upon metals, and corrodes the linen which is used for dressings.

In the second stage, the tubercles are situated more deeply in the cellular tissue; inflammation attacks the glands and lymphatic vessels, which are situated in this part. The irritation increases, when the patient is not submitted to proper treatment; and the pianic ulcers, instead of secreting a liquid like pus, only furnish an acrid matter, of a dirty yellow colour, spreading a particular odour, almost similar to that which passes from ulcers of a syphilitic kind.

In the last stage, the tubercles degenerate

into phagedenic ulcers; these are, at first, elevated, hard, and irregular, and give out a copious suppuration, which considerably reduces the strength of the patient; the inflammation is communicated to the pituitary membrane, which secretes an acrid fluid, and soon ulcerates. The ulcerations of the skin extend to the cellular tissue. The bones become carious, the breath fœtid, puriform expectoration takes place, pain in the abdomen manifesting inflammation of the viscera, a series of symptoms supervenes, announcing speedy death. On examination after death, the parts present a yellowish hard appearance; the epidermis is considerably thickened, and covered with small tubercles of irregular form, containing a glutinous liquid. In the cellular tissues, there are frequently observed depositions of tuberculated matter, having a white, or cheesy appearance. The glands and lymphatics are enlarged and distended, and in part ulcerated and disorganised. In some cases of this affection, the viscera have been found converted into a scirrhus mass. This disorganisation sometimes also occurs in the cellular tissue, muscles, ligaments, and cartilages; the bones are frequently found carious.

Treatment.—It is best, in general, to place the patient in a dry and elevated spot, at a distance from any marshes, where he may take exercise in the open air, which contribute in a very great degree to the cutaneous secretion, and the development of the eruption. All intercourse with persons attacked with pian, should be carefully avoided; the diet should be changed; animal food should not be given, if the patient be strong; if, on the contrary, weak, it will prove very beneficial.

Therapeutic Treatment.—At the commencement of the disease, the surgeon should have recourse to bleeding, if the strength of the patient permit. The use of sudorific drinks, as the decoction of sarsaparilla, or guaiacum, often produces beneficial effects. The juice of these plants, that of vervain, taken at a dose of two ounces every morning fasting, in equal parts of milk, may prove highly advantageous; the use of purgatives, the warm bath, and steam bath, frequently prove beneficial. Sulphureous waters may also be employed. These remedies should be continued until the patient be completely recovered.

If it happens that some tubercles degenerate into spreading ulcers, the same treatment must be adopted, and the ulcers covered with lint, dipped in the chloruret of soda.

Most writers agree as to the treatment, and think that the oxymuriate of mercury is the best remedy which can be employed. I am, however, of opinion, that mercury not only produces little good in warm climates, but that it is injurious from the effects which it produces in the system, by predisposing the mucous membranes in general to inflammation, and the glands to a state of scirrhus or induration.

I conclude that this remedy ought not to be prescribed in the treatment of pian. As soon as this disease manifests itself, no time must

be lost, and the practitioner must not wait, as some advise, for the commencement of the drying of the pustules. Pian may attack the same individual several times; and, indeed, the person who has once been affected with the complaint, is liable to a recurrence of it. Pian is less severe in whites than negroes.

The complaint is contagious; it passes from negroes to whites; it is, however, of less frequent occurrence in the latter, and does not spread so easily as syphilis. I know several whites, who have had frequent connexion with negroes infected with frambœsia, but who never caught it. This opinion does not coincide with that of most authors, but it is founded on numerous facts, which I have myself observed.

Alibert, in his splendid work on diseases of the skin, has described one, which he calls *pian ruboide*. I think, however, that this is not a pianic affection, but a sycosis of the scalp, as has been observed by Dr. Bateman. Dr. Bateman asserts, that the pianic eruption is generally observed on the scalp. I have seen and treated a great number of persons affected with pian, but I never saw it on the scalp.

Dr. Winterbottom says, that the tubercles are filled with an opaque and whitish matter. Before the tubercles break, no matter can be observed in them. When there is a depression of the skin, there is an ichorous discharge. Dr. Winterbottom also says, that it continues in children from six to nine months, and that adults are seldom cured in less than a year, and sometimes not in two or three years. Pian may be cured in three months, when it is attacked at its onset, and proper treatment is employed.

[Two cases of frambœsia are related, which were cured by warm bath, venesection, sudorific drinks, and mild purgatives. Nomercury was employed.]

From the London Medical Gazette.

STRANGULATED HERNIA IN AN INFANT TWENTY DAYS OLD.—Operation.

Treated by M. Dupuytren.—Hotel Dieu.

On the 14th of March, a male child, twenty days old, was admitted, presenting all the symptoms of a strangulated hernia: a shining and painful tumour in the bend of the right groin, descending to the scrotum, nausea, hiccough, stercoraceous vomiting, but not a complete suppression of stools. Leeches were applied, the infant was put into the warm bath, without success; the mother declared that this condition had lasted for three days. M. Sanson, who caused the child to be brought to the hospital, had no doubt of the existence of a hernia. M. Dupuytren examined the case with great care: the taxis caused a portion of the tumour to re-enter, which reappeared the moment the compression of the fingers was removed: uncertain as to the line of conduct that ought to be pursued, M. Du-

puytren caused the infant to be taken to the amphitheatre, and proceeded to a fresh examination of the parts, preceding it by some general considerations, of which the following is a summary.

Inguinal hernias, according to the professor, are not very uncommon at this age, and even in female children the proportion of these hernias is greater than of crural, which he thinks may be attributed to the want of development in the pelvis; but what may be considered as very rare at so tender an age, is a strangulation so marked as to call for an operation. The present is the only instance of the sort which M. Dupuytren has met with in so young a subject; the testicle appeared to have descended into the scrotum; it is situated at the lower and posterior part of the tumour, which at its lowest part appears to contain a fluid, which extends along the cord, as far as the superior part of the inguinal canal, which it entirely fills.

Some cases, in which, from the symptoms, the existence of a strangulated hernia has been suspected, and in which the operation has proved the mistake of the surgeon, must be taken into consideration; three or four times a swelling of the cord has imposed upon the operator; lately, indeed, the professor was called upon by M. Husson to visit a female in one of the wards of this hospital, who suffered from the symptoms of strangulated hernia, and who had in her right groin a voluminous tumour, in which M. Husson believed that he discovered a fluctuation, and which he thought to be an abscess: M. Dupuytren being of the same opinion, opened the tumour; an enormous quantity of pus escaped, and the apparent symptoms of strangulation disappeared. In the case at present under consideration, the strangulation, if it exists, is made by the neck of the sac, and the intestine must have escaped at the moment that the testicle passed through the ring, and carried with it a portion of the peritoneum forming the tunica vaginalis. After a moment's hesitation, M. Dupuytren decided upon operating, this appearing to afford the only chance of cure, and not being likely, in the event of a mistake, to aggravate the infant's state. But before he did any thing to the upper part of the tumour, he proposed to make an incision in the lower portion, opposite to the spot where he felt the fluctuation: the tumour once opened, if the intestine was found, it would not be difficult to prolong the incision, and disentangle it.

This incision was made, and gave issue to a quantity of serum; the testicle appeared bare, known by its white colour; a knot of intestine appeared above; its brownish colour contrasted with the white of the testicle, and left no doubt as to its nature: the incision was then prolonged upwards, and the whole tumour laid open; the intestine was kept down by the fingers, and the division of the stricture made with a blunt-pointed bistoury directly upwards. But here an unexpected difficulty presented itself: the infant continued to cry incessantly, and thus to contract its abdominal

muscles; and it became impossible to reduce the intestine, which even escaped in greater quantity. The difficulties in reducing the intestine, the necessity of renewing the attempt several times, the small extent of the parts, and the muscular contractions of the child, rendered this operation long and painful. Before and during the operation, the child had several abundant stools.

After the operation, the faecal evacuations became perfectly re-established—there were no longer hiccough, tension, vomiting, nor pains in the abdomen; but, whether the first dressing was made without due care, or whether the necessity of retaining the parts within the abdomen, induced the operator to tighten the bandage too much at the upper part, to the neglect of the lower, we know not, but when the dressing was removed the testicle was found outside of the wound: the house-surgeon could not replace it; it had contracted adhesions which M. Breschet would have destroyed, but an erysipelas had developed itself round the wound, upon the thighs, legs, and loins. M. Breschet inquired whether straps of adhesive plaster had been applied, to which the erysipelas might well have been attributed—but they had not; and therefore it became a question to what it was owing. It appears probable, that inflammation was produced by the compression of the testicle: its appearance out of the wound, its contact with the external air, and the dressings, might have contributed to it: had that been the case, perhaps it would have been right to have destroyed these adhesions at once, and to have reduced it directly, in spite of the inflammation to which its protrusion had given birth, and which tended to support it: however this might be, leeches, emollients, and two blisters to the legs, diminished the erysipelas, and induce a hope of the fortunate termination of the case.

From the London Medical Gazette.

VACCINATION.

To the Editor of the London Medical Gazette.

Sir—That a very general anxiety now prevails in this country on the subject of vaccination, will not, I presume, be questioned by any competent observer. In every family that I visit, something constantly occurs to bring it on the tapis: questions are every where put to me, touching the extent and degree of its protecting power; and every where do I find the same anxious but laudable desire to acquire some portion of that information on these points, which the world gives credit to the faculty for possessing. Under this impression, I have thought that a few pages of your excellent publication might (with reference to both the classes into which mankind may be divided—those who take physic and those who give it) be beneficially occupied with a discussion on the principal topics which at present occupy the public mind in

reference to vaccination. We have ceased to talk about a spurious cow-pox, a phantom which disturbed so greatly the minds of our predecessors. We have happily ceased, too, to consider vaccination as the parent of those cutaneous complaints by which infantile life has always been, and will probably ever continue to be, infested. But the march of time, which has left behind these subjects of common gossip and professional debate, has opened to us some new and curious questions concerning vaccine influence; and to these I propose to limit my attention in the short series of essays now contemplated. Being desirous to make myself intelligible to all classes of your readers, I must beg the indulgence of my professional brethren if I am sometimes more elementary than would otherwise be necessary.

The first and most natural question which occurs to those who hear of the fact of small-pox taking place after vaccination, is—what is the *extent* of such occurrences? In other words, what proportion of the vaccinated are subsequently affected by small-pox in a well marked and cognizable form? Their second question usually has reference to the *severity* or *intensity* of such attacks. Every one feels that the answers to these questions involve the very pith and substance of the whole question, and they will deserve, therefore, the priority of discussion. The last report of the National Vaccine Establishment touches upon both points, but in so very loose a manner that I am tempted to doubt how far the reply there given will be deemed satisfactory by the public. With regard to the question of *extent*, they say, that “the number of those who *fall into* small-pox after vaccination, is not greater than that of those who formerly *died* by inoculation whilst that practice prevailed.” How the members of the vaccine board have arrived at this conclusion, I am unable to say; nor do I clearly see whether they mean that the *absolute* or the *relative* numbers of those who take small-pox after vaccination, is greater than the deaths in old times by inoculation. Mr. Edmonstone, in his recent work, entitled “Observations on Cow-Pox,” p. 150, has shown that, if every individual born in the three kingdoms was to be inoculated for small-pox, the number of deaths by that disease would be annually 700. But as inoculation was certainly not practised, at any period, upon more than one-third of the population, the deaths accruing in that manner could never have exceeded 233; and surely the Board could not mean to say that this is the amount of cases of small-pox after vaccination throughout the whole island. In the year 1827, I had 105 cases under my own care at the Small-Pox Hospital, besides several whom I attended at the St. George’s and St. James’s Dispensary, and others in private practice. To suppose that I had under my own care one-half of all the cases that occurred throughout the kingdom in that year, is manifestly absurd. The Board, therefore, probably meant to intimate that such was the *relative* number of

those who take small-pox to those who die by inoculation; in other words, that one out of 500 vaccinated persons are subjected, in after life, to small-pox. This, however, I have reason to consider very wide of the truth.

I have often thought that a fair ground of calculation, as to the extent of small-pox after vaccination, might be drawn from the following source. In the same year that I received 105 patients after vaccination, I vaccinated 3702 persons; that is to say, one out of every 35 may be said to have returned upon my hands. This calculation, however, requires some correction, from the consideration that other vaccine institutions do not, for the most part, receive variolous patients. Making a due allowance for this, therefore, I would consider that one in fifty will express, as nearly as our present knowledge authorizes, the proportion of vaccinated who subsequently take small-pox in a well marked cognizable form. Whether or not this proportion has yet reached its maximum, and whether the increased attention now paid to the performance of the operation is likely at some future period to *lessen* the proportion, are points on which I feel myself incompetent to argue at length. I would merely observe, *en passant*, that the proportion of admissions into the Small-Pox Hospital, of patients labouring under small-pox after vaccination, which rapidly increased from 1808 to 1822, has become stationary since that period. The proportion in this case, it will be observed, is not between those who take it and who do not take it after vaccination, but between those who have small-pox after vaccination, and those who undergo that disease without preparation of any kind, and it is now as one to three. This fact induces me to think that the maximum of vaccine imperfection, is now obtained, and that the probability is rather in favour of a *diminution* than of any increase in those unpleasant occurrences which have of late so strongly excited the attention of the public.

The second question relates to the severity or intensity of the attacks of small-pox occurring subsequent to vaccination; and here again we have the Vaccine Board expressing themselves in doubtful terms. "This disease," it is said, "is safe, though sometimes severe." If by this we are to understand that small-pox, after vaccination, never terminates fatally, which the word *safe* appears decidedly to imply, the assertion is directly at variance with their own former reports, with the experience of the Small-Pox Hospital, with that of several of our best writers, and with the documents published in foreign countries, where vaccination is said to enjoy, like a prophet, more honour than in its native country. In the National Vaccine Report for 1819, we find the following passage:—"Five cases have been reported to the Board, of vaccinated persons who have subsequently died of small-pox." In the report for 1820 it is stated, "In several of these cases the malady has been prolonged to its ordinary period, and in eight reported cases it has proved fatal." That the occur-

rences are afterwards satisfactorily explained without impugning the general efficiency of vaccination, is undoubted. Again, since my last Report of the Practice of the Small-Pox Hospital, dated Jan. 1, 1826, up to the present day, I have received 190 patients having small-pox subsequent to well ascertained vaccination, of which number five have died. Several of our writers offer their testimony to the same fact. Witness Mr. Edmonstone, of Newcastle, in the work just quoted, (a strenuous advocate of vaccination,) who informs us, that from the year 1801, to the 29th April, 1825, 42 cases were entered on the books of the Newcastle Dispensary with small-pox after vaccination, including both real and presumed, of which number five died, being in the proportion of 1 in 8. Lastly, we read in a valuable document recently published by the College of Physicians of Stockholm, that an epidemic small-pox raged throughout Sweden in 1824, in the course of which 560 persons perished, of whom 34 are reported as having been vaccinated effectually, and 69 doubtfully.

I have quoted these several authorities, not with the insidious design of injuring a cause which I profess to support, but merely to establish the fact itself which may next become the subject of more particular inquiry. It must be obvious to all who have watched the course of this dreadful disorder, (the small-pox,) that it proves fatal in two different ways: first, by the violence and extent of its own specific action; and, secondly, by calling into activity other latent diseases. The fact I believe to be, that small-pox very rarely indeed proves fatal by the first of these modes; but that it does occasionally prove fatal in the second, cannot, I apprehend, be doubted. Nor need this be a matter either of regret or surprise. When small-pox and its accompanying fever, once take possession of a weak, delicate, and scrofulous habit, or of one extremely plethoric,—when such a disease attacks persons recovering from other disorders,—when its inroads are synchronous with some other affection, say inflammation of the lungs or bowels,—he is a bold man who would attempt to set bounds to its injurious tendencies. The eye of the practised physician can, indeed, distinguish between the specific and the accidental modes of death in small-pox; but this nicety can never be made intelligible to the public mind, which, in the event of the patient dying within 30 days from the invasion of small-pox, will always be inclined; and not without some justice, to attribute the death to that cause.

From all I have read and seen, I would say, that the public are not so unreasonable as to expect that vaccination should prove a complete and never failing safeguard against the manifold dangers of small-pox. They well know the importance of a sound constitution in resisting its attack; and further, that in the "severe" cases which the Vaccine Board acknowledge, the result must, in some degree, depend on the skill with which the patient is

treated. All that, as it appears to me, the public requires, is an assurance, founded on clear data, that in its ordinary and regular course, small-pox, as it occurs after vaccination, is like a sore throat, or rheumatic fever, a safe disease; and that the deaths which it sometimes occasions are referrible to accidental circumstances, which no human foresight could have guarded against. But of the 105 cases of small-pox, after undoubted vaccination, which fell under my care last year, as already stated, I lost but one—a man vaccinated in Kent; and his death is attributable to the concurrence of small-pox of a severer kind than usual, with a diseased condition of the lungs which would alone have occasioned his death, in all human probability, in less than six months. That the general character of small-pox, as it occurs subsequent to vaccination, is that of a mild disorder, not implicating the great springs of life, is a fact fully established, and as well known to the public as to the medical profession. My experience among the out-patients at the Small-Pox Hospital enables me to say that such a complaint is not, in any degree, dreaded by the great majority of the lower and middling ranks throughout the metropolis and its neighbourhood. One woman applied to me, within the last two months, expressly for the *vaccine preparative*, and all who apply at the hospital are impressed with the belief, that a certain proportion of the vaccinated will, in after life, become the subjects of the mitigated, or mild small-pox, as they commonly call it.

The extent to which inoculation is now carried in London and in the country; the increasing practice of re-vaccination, with its advantages and disadvantages; the propriety of testing with variolous matter, and the probable sources of imperfection in the vaccine process, will form the subjects of my next communication.

I have the honour to be, Sir,

Your Obedient Servant,

GEORGE GREGORY.

From the Edinburgh Medical and Surgical Journal.

NOTICE OF THE APPEARANCES IN THE BRAIN of a Young Female who attained her seventeenth year without giving Indications of Sensation or Motion from Birth. Communicated by DAVIES GILBERT, Esq. M. P., President of the Royal Society.

The young person who forms the subject of the following notice, was somewhat prematurely born, and appeared at birth to be very feeble. She gave no indications of the usual instinct which guides infants to their natural food; and much difficulty was experienced in teaching her to take the breast. One eye was observed to be smaller than the other, which appeared to be of the natural size.

Except this excessive weakness in the muscular motions, and total deficiency of the ordinary sensations and appetites, nothing particular was remarked for several weeks after

birth, when convulsive fits took place, and increased in violence and frequency of recurrence till they exceeded one hundred in twenty-four hours. Every method that could be suggested by various medical attendants to counteract these fits of convulsions was tried without success.

She nevertheless continued to grow in length much like other children. But the defects in motion and sensation increased and became more apparent. Though she originally gave indications of sight, blindness came on; and cataract was observed in the eye originally of the natural size. The sense of hearing, if it existed at all, was very obtuse. Latterly it was impossible to doubt that she was quite deaf. Though she got a first set of teeth, these were not succeeded by others, so that they were temporary only. The sense of taste seemed to be tolerably perfect, as fruit, confections, custards, and other sweet articles were received with indications of pleasure.

She never acquired any thing like the power of voluntary motion; and she attained her seventeenth year without ever having held up the head, raised her hand to her mouth, or set her foot to the ground. She never was heard to utter any thing like an articulate sound; and a feeble cry or whine only was believed to indicate want of food. When this was gratified, especially with sweet articles, the countenance exhibited an expression of satisfaction.

The bowels were habitually very slow, so as to require the perpetual employment of medicine; and to this no temporary exception was ever remarked. She never acquired any command over the sphincters.

For some short time previous to death the weakness seemed to increase; and exactly on the day when she completed her seventeenth year, she expired in a manner so gradual, that the attendants thought her asleep, when she was really dead.

I understand that some indications of the menstrual discharge had been observed; and the usual marks of puberty were not wanting. I am not aware, however, that the *mammæ* had enlarged.

Her length must have been equal to that of many young persons of her age. The spine and limbs were distorted; and the person was much emaciated.

Inspection of the body after death unfolded the following appearances, which are given in the words of the surgeon who performed the dissection.

“On opening the cranium, the first thing which presented itself to observation was the very great distention of the vessels of the brain and of its membranes. The first longitudinal sinus was particularly gorged, and marked its course by a raised dark blue line extending across the *dura mater* from before backwards. We laid it open, and found it filled with coagulated blood.

“On proceeding with the dissection, we found some little water in the lateral ventricles; but this is often poured out after death

in the healthy subject; and as the contents of the ventricles were perfect, more particularly the optic thalami, I should be inclined to consider that as a natural appearance.

"On removing the brain from the basis of the skull, so as to expose the nerves, we could trace each pair taking their usual course to their destination; and they all appeared firm and healthy, as in the most intelligent individual. But in the base of the skull itself, not a vestige of the dura mater was to be seen. Its place was supplied by a thin semitransparent membrane, very lax and irregular, so that it afforded no protection to the nerves in their exit from the skull.

"On the back part likewise the whole, or the greater part of the *tentorium* was deficient, thus allowing the whole weight of the brain to rest on the cerebellum.

"We proceeded carefully to trace the nerves to their separate organs, and found them in every respect seemingly perfect. Indeed the optic nerves appeared particularly developed, as also the ophthalmic branches, although cataract existed in both eyes to a very considerable degree. Nothing further appeared worthy of observation. From these circumstances I should be inclined to say, (as far as human research can explain the mysteries of our nature) the causes of the child's deficiency were as follow.

"That from the want of the *dura mater* on the base of the skull, and its place being only imperfectly supplied by the thin expansion before mentioned, the mass of brain was allowed to press on the nerves of sense as they passed through their separate foramina, and thus their vital principle became destroyed; and that the cerebellum, not having any protection, in consequence of the want of *tentorium*, was also pressed upon by the cerebrum, which likewise suffered in its turn, thus probably accounting for the total extinction of intellect."

(Signed) JAMES COLES,
Member of the Royal College of Surgeons,
Feb. 1826. London.

The defect of *tentorium* and of the sphenoccipital portion of the *dura mater* in the case now related is important, in directing the attention to the probable influence of one part of the organ pressing or gravitating, as is said, on another. That this influence, however, may not be overrated, it is requisite to mention that Sir Anthony Carlisle published many years ago in the first volume of the Transactions of the Society for the Improvement of Medical and Surgical Knowledge, the history of a woman apparently near 20, who died with symptoms of fever in the Westminster Hospital. In the brain of this woman the *falx* was entirely wanting from the *crista galli* to the *tentorium*, the opening in which also was much larger than usual; and consequently, not only did the one hemisphere press the other, but part of the brain gravitated on the cerebellum. Yet in this woman the intellectual powers were said to be perfect; and she had no particular complaint of her head. Vol. i. p. 212.

From the London Medical Gazette.

CYSTS CONTAINING A WATERY FLUID, APPARENTLY CONNECTED WITH THE LIVER.

To the Editor of the London Medical Gazette.
Sir,

I am induced to send you an account of the two following cases, first, because I am not aware that any cases exactly similar to them have been recorded by pathological writers; and secondly, because I have lately had an opportunity of seeing one of the patients, and have thus been enabled to ascertain that the slight operation which was performed upwards of five years and a half ago has afforded her a permanent relief—or at least that up to the present period there has been no indication of any recurrence of the disease.

I am, sir,
Your obedient servant,
B. C. BRODIE.

Saville Row, Jan. 20.

CASE I.—In the spring of the year 1822, I was consulted, in conjunction with a respectable medical practitioner of my acquaintance, respecting a young lady about 20 years of age, who laboured under a considerable enlargement of the parts situated in the right hypochondriac region. On examination we discovered a tumour, in which the fluctuation of fluid was very perceptible, lifting up the inferior ribs of the right side, and projecting forward so as to be visible below the edges of the ribs also. This tumour had begun to appear indistinctly a year or two before; at first producing more pain as it grew larger, and occasioning also much inconvenience to the patient by preventing her from sleeping, except in a particular position, and from taking exercise.

The tumour continued to increase in spite of some remedies which were recommended; and as the inconvenience which it produced increased also, I was led, on the 27th of June, 1822, to puncture it by means of a flat trocar cautiously introduced below the margin of the ribs. About three pints of watery fluid were evacuated. Care was taken that no air entered the orifice made by the trocar. The edges of the wound were brought together by means of adhesive plaster, and they healed by the first intention.

A bandage was applied round the upper part of the abdomen, and the patient was kept in bed for the first few days after the operation.

For two or three weeks she suffered from a troublesome and almost incessant cough. As the cough subsided, she found herself to be very much relieved. She was now free from pain, was able to walk as well as other persons, and to lie in any position.

About a fortnight ago I was again consulted by the same young lady, but it was on account of another complaint, and I was gratified to find that she had continued quite free from any of her former symptoms.

CASE II.—In August, 1822, a little boy was admitted into St. George's Hospital, under Dr. Chambers, with a tumour presenting itself be-

low the margin of the ribs on the right side, lifting up the ribs also with a distinct fluctuation, and altogether very nearly resembling the tumour which existed in the case just related.

In the following September it was determined, in a consultation between Dr. Chambers and myself, that I should puncture the tumour. Accordingly, a flat trocar was introduced below the margin of the ribs, and a pint and a half of watery fluid was drawn off. The wound healed by the first intention, without the smallest inconvenience arising from the operation. The boy left the hospital as cured, and I have had no opportunity of seeing or hearing of him since.

The fluid drawn off was exactly the same in both of these cases. It was clear and colourless, resembling water in appearance; it contained no coagulable lymph; and when exposed to heat in a spoon over the flame of a candle, it almost wholly evaporated, leaving the smallest possible residuum of animal matter.

From the situation of these tumours I was led to believe that the disease must have been situated in the liver.

Cysts are found in the liver containing hydatids, but the circumstances of there being no *débris* of hydatids in the fluid drawn off, and of there having been in one instance, at least, no return of the disease at the expiration of five or six years, seem to be at variance with the opinion that such was the nature of the tumour in either of the above cases.

Not unfrequently I have noticed in dissection a membranous cyst projecting on the convex surface of the liver, containing a clear, watery fluid. The cysts to which I allude are generally of the size of a marble. The largest which I have chanced to see in the dead body might have contained from one to two ounces of water: but there is no reason why such a cyst should not increase so as to attain a very large size, and I can offer no better explanation than this of the cases which I have now recorded.

A late number of the above Journal contains the following case by Dr. Anthony Todd Thompson.

In the summer of 1813, I was requested to see the apprentice of a baker in Chelsea, who was supposed to be labouring under hepatitis, for which he had been twice salivated; once in an hospital, and again under the direction of a general practitioner. The boy was about 14 years of age, rather short in stature, of a sallow complexion and sickly appearance. He complained of no acute pain, and said he had never felt any; but he experienced considerable difficulty of breathing, and could obtain no sleep except in a nearly erect position. He ascribed this inconvenience to "a swelling at the pit of the stomach," to use his own words, "which had been coming for upwards of two years."

On examining the tumour, I found that it was situated under the margin of the ribs, towards the right side, and protruded forward in a

pyramidal shape, nearly the size of the human fist. It was tense and elastic, and afforded indistinct indications of containing a fluid. Although sallow, yet the boy did not appear to be jaundiced: the eye was free from any yellow tinge, the tongue was clean, the pulse regular and moderate, and the alvine discharges natural. As no benefit had been derived from the mercurial courses, nor from any medicines which he had taken, I resolved to open the tumour, and puncture it with a common hydrocele trocar. Upwards of three pints of watery fluid were evacuated, which slightly coagulated when a portion of it was heated in a spoon over the flame of a candle. The wound healed by the first intention without confinement, and without a dose of medicine having been taken. I conclude there has been no return of the disease, as the patient was desired to apply to me again should the tumour re-appear; and I have not seen him from that time to the present period.

I am inclined to believe, with Mr. Brodie, that the fluid, in these cases, is contained in a cyst attached to the surface of the liver.

From the London Medical Gazette.

Case, in which an adventitious Membrane was found adhering to the inner Surface of the Cerebral part of the Dura Mater. By T. H. HOLBERTON.

George Mason, æt. two years and three months, up to the age of ten months was a particularly healthy and forward child. At this period he cut the incisor teeth of the lower jaw, and had the slight fever which generally accompanies dentition, but in so trifling a degree, that the mother merely gave him an occasional dose of opening medicine, not thinking his illness of sufficient importance to require medical advice. The child, however, grew worse; and a month afterwards had the first of a series of fits, which continued to recur with more or less frequency during the remainder of his life. By the fifth month from the attack, the child then at the age of fifteen months, lost all knowledge of its parents, took no notice of external objects, and lay in a seemingly insensible state, being unconscious to the calls of nature. He felt, however, sensations of hunger, and signified by his cries when he desired food; yet he could not perceive the presence of it till it was placed on his lips, when he usually ate it with a good appetite. He could move his arms and legs, but could not stand.

When I first saw him he was clearly labouring under compression on the brain. Pupils widely dilated, insensible, and immoveable on the application of a lighted candle near the eyes. There was strabismus, eyes rolling in every direction, particularly from above inwards; the breathing was natural; there were frequent convulsive fits; the bowels were regular. He had during the last week or ten days of his life, frequent twitchings of the left leg.

Examination after death.—The skull-cap was preternaturally thickened, but the head was of the ordinary size. On opening the dura mater there was found upon its under surface a membrane for the most part three lines in thickness, and closely attached to it. This membrane completely lined the cerebral surface of the dura mater, excepting only at the fore-part of the base, viz. at a short space around the optic nerves. Underneath this membrane, and between it and the arachnoid, was contained rather more than half a pint of serum. There was no collection of fluid at any part between the false membrane and dura mater. The adventitious membrane appeared perfectly organized; its unattached surface was very vascular, and in parts much inflamed: this surface admitted of being separated as a distinct and continuous membrane, extremely fine and transparent.

The arachnoid membrane covering the brain was, perhaps, in a slight degree, firmer and thicker than usual. There was some effusion between it and the pia mater.

The pia mater on the surface of the brain was preternaturally vascular, as was that portion dipping between its convolutions.

The choroid plexuses were gorged with blood, and resembled a dark coagulum of a shining, purplish hue, approaching to the colour of a well-ripened black grape.

The cerebrum generally was very much harder, and the convolutions were much wider than natural. The medullary portion moreover was altered in colour, being of a yellowish tint; behind the lateral ventricle the substance of the brain was softened.

The cerebellum was much firmer than usual, so as to render the laminated structure beautifully distinct.

The adventitious membrane was not present at the under surface of the tentorium, nor around the cerebellum at any part; it adhered only to that part of the dura mater which surrounds the cerebrum.

The preparation is in Mr. Mayo's Museum, Great Windmill Street.

From the London Medical Repository.

MALA PRAXIS.—ROLFE V. STANLEY.

A case of great interest to medical men was tried in the Court of Common Pleas about a fortnight ago. It was an action brought by a person named Rolfe against Mr. Stanley, Assistant Surgeon to St. Bartholomew's Hospital, to recover damages for an alleged neglect and want of surgical skill in the treatment of an injury of the knee-joint. We shall lay before our readers the evidence given on the occasion, as well as the charge of the learned judge to the jury, and leave them to form their own opinion of the merits of the case.

Mr. Sergeant Cross stated the case, and called the following witnesses:—

Mr. Henry V. Garman stated that he is a surgeon-apothecary. On the 2d of December,

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1826, he was in the neighbourhood of Mile-end road, and was called to attend the plaintiff. He found him in a cottage about half an hour after the accident occurred. He appeared to have received a laceration of the right knee-pan, in which there was a hard and moveable substance. He washed away all the superficial particles, and, after putting on some simple application, he took him home. He went first for Mr. Green, the surgeon, but not finding him at home, called for Mr. Stanley, who resides in Lincoln's Inn fields. He came in three hours after, and the patient's knee was more swollen. Mr. Stanley appeared to me to have made a proper examination, and he requested me to put the limb in splints, to prevent the motion of the joints, and keep the limb quiet.

Mr. James Rolfe, the father of the plaintiff, said he was not present when Mr. Stanley examined the plaintiff's knee, but when he came down stairs he prepared him for the worst. He said the plaintiff would have a stiff joint as long as he lived, and that he might be glad if nothing worse occurred. He added, that there was a shocking fracture, and about the fourth or fifth part of the knee-pan was broken off. The splints remained on for five or six days, when Mr. Stanley took them off and bent the knee, which caused such excruciating pain, that the plaintiff was obliged to request him to desist. When the splints pressed on the hard substance, it made him quite sick with pain, and he was always pushing them down. When this was told to the defendant, he said the patient must have patience. Mr. Stanley used sometimes to attend twice a-day for the first six days, and when spoken to on the subject, he said the knee-pan was broken, and the hard substance felt in that part was a portion of the knee-pan, broken off. On the 18th January, he said he could not do any more for him, and that his cure depended more on himself than upon him, the defendant. When the plaintiff got well enough to be removed, he was sent to Hammersmith for a change of air. The wound then swelled as large as his head, and discharged a quantity of matter, in consequence of which it was necessary to call in Mr. Lilly. The plaintiff was not able to attend to business during this time.

Cross-examined—He did not walk to see the Duke of York's funeral, but went in a coach. Mr. Stanley discontinued his visits about that time. Mr. Stanley might have called on that day, but he could not speak positively. The plaintiff's principal object for going to Hammersmith was to see the Duke of York's funeral; he also thought the change of air would do him good. During his absence Mr. Stanley called twice. The plaintiff came back in a fortnight from Hammersmith.

Re-examined—Mr. Stanley said he might take moderate exercise as soon as he was able; his son always pointed out the knee-pan as the seat of pain.

Examined by the Judge—He did not think Mr. Stanley had notice of the discharge in the plaintiff's knee.

Mr. Charles Lilly is a Member of the College of Surgeons. He was called in to attend the plaintiff in September; he found him very feeble; there was a hard moveable substance in the knee-pan, near the surface. He examined it, and advised soothing poultices to be applied, and the patient to be kept quiet. In a month his health was very much improved, and on examining the knee at the end of the second month he found the hard substance had advanced so near the surface that one point of it had come through the wound. He enlarged the orifice, and extracted the flint now produced, and afterwards removed a second piece of flint; a third portion came out in the dressing of the knee. The pan was certainly not fractured; and if it had been broken, that could undoubtedly have been discovered.

Cross-examined—When first he saw the knee, he thought the hard substance was bone. He could not imagine where it came from, or pretend to say how it arose. Exercise, he supposed, would tend to bring the hard substance to the surface, but nature would, of course, have the same effect; if the mode of treatment required that no probing should take place, the same course would be adopted whether there was a piece of bone in the knee-pan, or a foreign substance.

Re-examined—It would be impossible to say, from the appearance of the scar, if it was large enough to admit the piece of flint. If that had been taken out in the first instance, the wound would have been healed when he first saw it.

Mr. Sergeant Taddy addressed the Jury for the defendant, and called the following witnesses:—

Sir Astley Cooper, Bart.—I have attended to the accounts given by the different witnesses, and, having given the subject all the attention I am capable of, I cannot discover any want of skill or attention on the part of Mr. Stanley; and if I had discovered a hard substance under the same circumstances, my mode of treatment would have been the same as Mr. Stanley's. The present was a very doubtful case, and one which required great caution. No man is more informed in anatomy than Mr. Stanley. Probing the wound at that time might have destroyed life, or, at least, occasioned the loss of the limb; for the smallest wound made in the knee-joint, such as that made by probing, might have destroyed life. I have seen an opening in that part less than that which might be made by a crow-quill, and such opening, to my knowledge, destroyed life in less than a week. If an incision had been made, or probing used under these circumstances, he (the surgeon) would probably have opened the knee-joint in doing so, and in that way would have inflicted some serious injury upon the limb, and this latter mode of treatment ought not to have been followed under such circumstances. A fall from a horse would be likely to break the patella, and it is the only way I have known longitudinal fractures of the patella occur. The natural process for a hard substance (as in the present

case) is to approach the surface of the skin. I think Mr. Stanley a most complete judge in such cases; and if I had been in attendance upon the patient, I should not have varied the treatment in any way from that followed by the defendant.

Cross-examined—I consider myself capable of saying what treatment ought to be used in a case I had not seen, supposing all the circumstances are afterwards detailed to me. I have heard the facts from Mr. Stanley before I came into court; he called on me and mentioned the circumstances, and I gave my opinion on his statement of the case; but my opinion has been very much influenced by the evidence I have heard to-day, and particularly by the clear and distinct statement of Mr. Lilly. I believe the knee-joint was not broken. If I had been called to attend a patient who complained that a certain spot was the seat of constant pain, I should (if any new circumstances occurred) look again at the wound; but, if I had made up my mind from the beginning, from my knowledge of anatomy, I would not alter the mode of treatment; it would be very dangerous to be constantly removing the bandages, and I should say that the man who would frequently examine fracture, and remove bandages for that purpose, would be a very unskilful surgeon, and extremely unfit to have the care of a patient.

Mr. Sergeant Cross—There was no fracture in this case; I therefore wish to know from the witness if, in case the patient frequently pointed out a part as the seat of pain, whether he (Sir A. Cooper) would not have examined that part a second time, in order to see if there was any thing which required further consideration?

Sir A. Cooper—If I thought I could relieve pain by again examining the wound, I should do so; but I would examine a case of this kind, in the beginning, and, having made up my mind on the nature of it, I would not vacillate in my opinion, or alter the general mode of treatment.

Re-examined—Mr. Stanley did every thing a good surgeon could do, and left nothing undone. This is my opinion, putting out of my mind all I have heard out of Court, and judging only by the evidence adduced to-day. I must say that I came into Court with some doubt; but it was the evidence of Mr. Lilly that made me as firm as I am at present.

Mr. Brodie stated, that he is a surgeon of St. George's hospital. He had been in Court since the opening of the case; and, judging by the evidence alone, he was convinced that Mr. Stanley had shown no want of skill or attention. A fracture of the patella is not uncommon when a fall from a horse has taken place. It would not be easy, under the circumstances, to distinguish a piece of bone from a foreign substance. If witness had been called in he would have followed the same mode of treatment as that pursued by Mr. Stanley, whom he considered a skilful anatomist and experienced surgeon.

Cross-examined—If he had been employed

to attend the patient, he would have kept him as quiet as possible, and done every thing to prevent inflammation. He does not recollect any thing else he would have done on the occasion. A person who saw the wound eight or nine months after the accident, might be better able to judge of it than at first; and a surgeon who examined it three hours after it occurred would see it to a disadvantage, as the part must then be considerably swollen. He heard several accounts of this case. The first was from a friend of the plaintiff; the second from the defendant; and the third from Mr. Lilly. From all he heard on the subject, he did not think that any blame was attributable to Mr. Stanley.

Mr. Travers and Mr. Green, surgeons at St. Thomas's hospital, concurred in stating that the defendant had pursued a skilful and proper course of treatment in the present case, and that no blame could attach to him.

Mr. Bell, an eminent surgeon, said the defendant had used all due skill and attention in the mode of treatment above referred to.

Cross-examined—Some doubt might have been caused in his mind by the pain not subsiding, and the hard substance not adhering to the knee-pan after two months.

Mr. Abernethy was next examined, and he stated that if he had been called to attend the plaintiff, he would have followed the same mode of treatment used by Mr. Stanley, against whom he saw no cause of complaint on the ground of want of skill or attention.

Mr. Sergeant Cross having replied,

Mr. Justice Burrough summed up the case, and observed that if the Jury were of opinion that the defendant had been deficient in point of skill, or negligent in his attention to the plaintiff, they must find a verdict for the latter; but if they were of opinion due care had been used, and proper talent exercised, the defendant was entitled to their verdict. Some of the first surgeons in Europe all agreed in saying they would have done as Mr. Stanley had done; and, what was remarkable, they all said it would have been dangerous to have made an incision at the time; but, considering the evidence of the very learned surgeons who were examined, the plaintiff's case was extremely weak. If they (the Jury) believed what these witnesses said, he could not fancy but their verdict would be for the defendant.

The Jury, after retiring for an hour, returned with a verdict for the plaintiff.—Damages 30*l*.

Medical and Philosophical Intelligence.

On the tendency of Matter to become organized.—We noticed, on former occasions, M. Bory de St. Vincent's observations on those ambiguous beings, which, during a part of their life, are collected into filaments, whose colour and general aspect are those of vegetables, and which, at certain periods, separate and assume the voluntary motion of animals. M. Gaillon, an enlightened observer, the author of an interesting memoir on the cause of the green colour in oysters, has discovered that it is produced by the *Conferva comoides*. He has seen the greenish corpuscles, which form its axis, become detached, advance with more or less rapidity, change place, and, in short, act in all respects like enchylides and cyclidia. Taking entire filaments, he forced these minute beings to separate before the time; and, in this case also, they manifested the same voluntary movements. Their propensity to associate is so great, that, whenever the young can do so, they arrange themselves, one after another, in a single line; and, when in this position, M. Gaillon thought he observed them to exude from their substance a mucosity, which forms itself into a membrane, and entirely envelops them*. M. Bory de St. Vincent has con-

tinued to occupy himself with these microscopic transformations, having in view to penetrate to the first combinations of matter to which these corpuscles seem so near. Observing the appearances successively presented in water exposed to light, he thought he saw, for the first time, matter assume the aspect of a simple mucosity, without colour or form. If the water contains any animal substance, it produces a pellicle of this mucosity at its surface, then becomes turbid, and discloses an infinity of living atoms, if we may so call those monads, which, after being magnified a thousand times, are not so large as the point of a needle, and which yet move in all directions, with prodigious velocity. This is what M. Bory names matter in the living state. When the water is exposed to the air and light, there quickly forms what is named the green matter of Priestley, which many observers have supposed to be the first state of certain confervæ, or plants of a like nature. M. Bory thinks that it is a combination of a more general form, and only susceptible of entering into the composition of these plants, as well as of the animalcules which issue from it, and which produce them. He names this combination *matter in the vegetative state*. It is by it that the infusory animals are rendered

* M. Mertens, a botanist of Bremen, has observed similar facts on the *Conferva mutabilis*. On the 3d August, he says, it was in its vegetable state; on the 5th it resolved itself into molecules possessed of mobility; on the

6th some of these molecules united into simple articulations; and, on the 11th, it was restored to its original form.

green. Those which colour oysters, according to M. Gaillon's observations, produce this effect, as M. Bory says, only because they are themselves coloured by the green matter. It colours, in the same manner, the water and the shells of these oysters; and it would not be impossible to find some tinged directly by this matter, without any animalcules having penetrated into them. It is so difficult to render observations of this kind complete, and one may always so easily suppose an anterior state, still more attenuated, and which may have escaped every microscope, or invisible germs, which the necessity of the concurrence of air prevents from separating, that many philosophers will probably refuse to admit the consequences, which the author would draw from these facts, for attributing to matter a general disposition to become organized, which would be independent of the ordinary mode of generation.—*Mem. de l'Acad. Roy. des. Sc.—Ed. New Phil. Jour.*

Disease of the Heart caused by Onanism.—Dr. Krimer, of Aach, has lately published an interesting paper on this subject. Our own experience has furnished us with several opportunities of seeing cases of the kind he describes; and as the subject has not hitherto been particularly discussed; we shall give the leading points of his communication. Dr. K. is of opinion that diseases of the heart, which have increased so much within the last twenty years, do not always depend upon organic alteration, but are very frequently produced by the baneful and lamentably frequent practice of the vice of onanism. Headachs, great anxiety, palpitations, faintness, an oppression and unusual sensibility in the epigastric region, are the first symptoms produced. They increase in severity in proportion as the subject gives way to the gratification of his unnatural propensity, and quickly diminish, or cease altogether, if he abandons it. To support his opinions, M. K. states many cases. He enumerates the following symptoms as pathognomonic of such affections of the heart; by an attention to which, the practitioner will be enabled to distinguish the train of symptoms from other diseases which are not unfrequently suspected.

1. The hair loses its natural brilliancy, is remarkably dry, and frequently splits at the extremities. It falls off easily and in large quantities, especially from the fore part of the head. In persons affected with consumption, or organic disease of the heart, the hairs appear well nourished, and rarely fall off.

2. The eyes are dull, downcast, frequently full of tears, and without expression, and deeply sunken in their orbits. The edges of the eyelids are reddish, and surrounded by a bluish tint. In phthisical patients, and those with organic disease of the heart, the eyes are brilliant, and always preserve their natural expression and vivacity. In young females, at the approach of menstruation, a blue circle is commonly observed around the eyes, but here also their brilliancy is undiminished.

3. The patient appears very timid, and unwilling to look other people in the face.

4. Periodical headach is common, extending from the occiput towards the forehead.

5. The power of sight is diminished; the appetite is lost; the tongue is usually loaded. A slight cough, and difficult respiration, are generally present; but still the patient can draw a deep inspiration.

6. Pains in the stomach, with weight and pressure in the epigastric region. Patients with organic diseases of the heart have occasionally these symptoms, but in such cases, they are not accompanied by those above enumerated.

7. A general feeling of lassitude, and feebleness of the limbs, with pains in the lower part of the back. We would add also, that pain and throbbing of the testicles, with uneasy sensations shooting up the spermatic cord, are frequently complained of.

8. The perspiration has a dull and sweetish odour, similar to that of infants at the breast.

9. If the vice of onanism be touched upon in conversation, the agitation and embarrassment of the patient invariably betray him.

10. If the practice be continued, the mind is at length enfeebled, the patient is incapable of mental or bodily exertion, and sinks into a state of somnolency.—*Hufeland's Journ.*

Period of the Disease at which Measles begin to be infectious.—Various opinions have been entertained regarding the period at which small-pox, measles, and other exanthematic diseases begin to be capable of propagating themselves. It is unnecessary for our present purpose to particularise these opinions and the foundation on which they rest. It is enough to mention that many believe measles in particular not to be infectious till the eruption has broken out. The following observation lately made by Dr. Harnier of Pymont establishes satisfactorily the contrary. A London merchant set off from London for Pymont on the 30th June 1825, along with his wife and three children, leaving behind, under the charge of a nurse, a fourth child, who had been attacked the day before apparently with catarrh. The party arrived at Pymont on the 8th of July, and had not been long there before they heard from London that the illness of the child left behind had turned out to be the measles, the eruption having appeared on the 1st July. Meanwhile one of the children at Pymont, the play-fellow of the one left behind, had been taken ill on the 9th with what appeared to be a common catarrh, and the rest of the family were not kept apart from him till the 11th. On the 12th the eruption of measles broke out. On the 24th and 25th of the same month the two other children, and three little girls belonging to the house where the family lodged, likewise fell sick, and in due course of time all of them had the eruption of measles. The five little patients had not seen the second child after the 11th, *the day before*

the appearance of the eruption, and the three girls of the Pymont family had even been sent out of the house altogether. There was not a case of measles at the time in Pymont, and in consequence of the strict seclusion of the different patients, the disease did not spread.—*Edinburgh Med. and Surg. Journal.*

Rupture of the Pulmonary Artery.—This very rare occurrence is described by Mr. Adam Junior, in the Calcutta Transactions. It took place in a native of Switzerland, whose constitution was worn out by intemperance and a thirty years' residence in India. The patient died suddenly, though for two days before he had suffered from paroxysms of acute pain in the right hypochondrium, accompanied with difficulty of breathing, so as to render a change from the recumbent position intolerable. But during two years he had had an intermitting pulse, palpitation, and difficulty of breathing. On dissection, the right cavity of the chest was found to be filled with blood. The walls of the right ventricle were very thin, the valves of the artery were in a state of ossification, its parietes extremely thin, even dilated to four times their usual size, its circumference being not less than seven inches. It contained an organized polypus, extending from the semilunar valves about four inches along the artery. The rupture had taken place in the middle, and on farther examination it appeared that there was an opening in the centre of the polypus, through which the blood seemed to be transmitted from the ventricle to the lungs. The aorta and its branches were highly ossified.—*Edinburgh Med. and Surg. Journal.*

Death caused by the external application of Opium.—The application of opium in considerable quantity to the skin when deprived of its cuticle is a practice by no means free from danger; and when it is resorted to for allaying irritation, it is necessary to watch the patient carefully. We are acquainted with an instance in which deep coma was induced by the application of an opium poultice to the scrotum previously deprived of the cuticle by a blister, and which would in all likelihood have proved fatal, had not one of the patient's friends called accidentally to see him and discovered the cause of his state of sopor. The following is an example of death originating in the same source, although the skin was not deprived of its cuticle. A soldier, thirty-two years of age, was attacked with phlegmonous erysipelas of the fore and outer part of the right leg, on account of which a linseed poultice, moistened with fifteen drops of laudanum, was ordered to be applied to the limb. Next morning he was found in a state of deep sopor, with the face pale, the eyelids tremulous and half open, the pupils contracted, the lips distorted, the muscles of the face affected with spasm, and those of the limbs with convulsions. The surgeon having remarked a strong odour of opium, as well as a

yellow coloration of the bandage which enveloped the limb, he had it removed, and found not only the bandage but all the compresses soaked with laudanum, which was flowing copiously from the poultice. The attendant whose duty it was to make up the poultice had left it in charge to an hospital servant without telling him what quantity of laudanum was to be used; and the latter had ignorantly used towards an ounce. Antispasmodics, emetics, and sinapisms, were resorted to, but in vain. The convulsions increased, the pulse became more and more feeble, and the patient died. On inspection some red points were seen on the arachnoid, a strong opiate odour was exhaled from all parts, and the heart, stomach, and brain were in their healthy state. In the blood-vessels no trace could be found of the poison which had been absorbed.—*Journal de Chimie Medicale.*

Case of Double Uterus, with Double Impregnation.—An extraordinary case of this description has been related by Dr. Geiss, a physician of Traffurth near Erfurt. The woman had been in labour for two days, when he was requested to attend her. He found her a stout, healthy female, of middle stature. The labour-pains were confined to the right side. On the right side the womb reached as high as the thorax; on the left it did not extend above the navel, and inclined forwards and laterally. The external parts and os uteri were properly constructed, and the shoulder of the child could be felt behind the membranes. The operation of turning was performed, and a healthy female infant delivered, upon which the right side of the abdomen sunk in while the left continued prominent. In an hour the labour-pains recurred, and on examining the state of the parts, Dr. Geiss found, that after passing the os uteri, the finger came in contact with a membrane distended by fluid, and protruding through an annular opening towards the left side, while the navel-string of the child already delivered, extended upwards into a cavity like the uterus in ordinary circumstances. On farther examination, he found the belly of a second child presenting at the accessory orifice, and therefore performed the operation of turning, and brought into the world a stout still-born male child, which was soon resuscitated. As no placenta followed for some time, he proceeded to assist the separation of them, and had then an opportunity of completely satisfying himself that his patient had a double uterus. The right placenta came away first, and the right womb contracted vigorously; the left placenta followed, but the uterus of that side contracted slowly, and the woman therefore lost a good deal of blood. Two months afterwards the mother and both children were in good health. Two years before, she had brought a single child, and the labour was tedious, and required manual aid to accomplish.—*Rust's Magazine.*

Rare Aneurismal Disease of the Temporal Arteries.—The following is an account of the dissection of the case recorded under the above title in the February number of the Monthly Journal of Foreign Medicine. The patient was admitted, a short time since, into the Middlesex Hospital, for lumbar abscess, of which he eventually died. The tumour at the time of his death was large, and pulsated very strongly.

On dissection, the posterior temporal artery was found to divide into two branches, one running, as usual, beneath the skin, the other perforating the tendon of the occipito-frontalis, and then, considerably enlarged, twisting and turning and coiling upon itself, looking, when injected, as like a varicose vein, as it is possible for a vessel which has no valves, to resemble one which has them. On examining the neck, the common carotid was obliterated, and converted into a cellular cord just up to its bifurcation, whilst the jugular vein was in nearly the same state of degeneration from the site of the ligature up to the division of the carotids, from which to the base of the brain it was plugged with coagulum. There was much pus at the basis cranii, and around the commissura tractuum opticorum; it lay between the pia mater and arachnoid, and was continued within these membranes down the whole length of the spinal cord. The cranium was somewhat grooved beneath the tumour, just as it would be beneath a powerful muscle, but the pericranium was entire, and the inner surface of the bone was *perfectly smooth and equal.*—*Med. Ch. Rev.*

Diseases of the Placenta.—The cause of death in still-born children is seldom inquired into, and there is, perhaps, no branch of pathology involved in greater obscurity than that connected with the fœtus and its appendages in utero. Death is generally attributed to some malady affecting the child itself, neither understood nor sought after, or else to a separation of a part, or the whole, of the placenta from the surface of the uterus. It is seldom that the condition of the placenta itself is examined, and it is perhaps as seldom that the child undergoes any inspection. That the placenta is liable to disease, like every other vital part, is sufficiently proved by the fact, that a part of it is not unfrequently found converted into a cartilaginous or osseous state. It might be inferred from this that it is subject to other diseases, and that these diseases must frequently be the cause of the death of the fœtus. That this is the case, M. Brachet, physician to the Hotel Dieu, clearly shows in the Journal Général de Médecine, where he relates several cases which came under his own observation.

A woman was delivered for the first time, of twins, one of them being alive and the other dead. She continued well during her pregnancy, and the labour was natural. M. Brachet having examined the dead fœtus, and finding nothing in the three splanchnic ca-

vities to account for its death, was led to examine the two placentæ. The one belonging to the living child was of a reddish colour, and of a moderately firm consistence; and on pressing it, nothing but florid blood ran out. The cord was healthy. The other placenta had a livid colour, it was voluminous, of a very firm consistence, and its tissue was compact, yielding with difficulty to pressure; but it was more friable than that of the other, and easily torn. On being cut into, its appearance was like that of the red hepatization of the lungs, and on its surface being scraped, it gave a thick reddish fluid. M. Brachet attributes the death of the fœtus to the disease of the placenta, which disease being regarded analogous to inflammation of the lungs, he terms inflammation of the placenta.

Madame P— had a fall in her seventh month of pregnancy. After this she felt great pain in the abdomen and loins, and she was obliged to keep her bed for three weeks. Seven weeks after the accident, she was delivered of a very lean, feeble child. On the fœtal surface of the placenta there were found two white, foul tumours, about the size of pigeon's eggs, each of which contained a quantity of lactescent fluid, having all the characters of pus. The following year, Madame P— was again delivered of a dead child, whose abdomen was of a greenish hue, deprived of its epidermis in its passage through the pelvis. The placenta was very voluminous; three-fourths of its extent was occupied by a large depôt, which contained more than a glassful of purulent matter mixed with blood.

M. Brachet relates eleven cases where the placenta was found diseased, and the fœtus was either dead or in a feeble state in each. This is a subject well worthy of attention, for we believe very little has been yet paid to it. —*London Med. Repos.*

Rupture of the Vena Cava Inferior.—Dr. Merry was requested to visit a young man, who, it was stated, had fallen down in a fit. He found him extended upon the floor, in a state of insensibility. The usual means, bleeding, the administration of brandy and other stimulants, were immediately employed, but without the least effect.

The body was examined 24 hours after death, in presence of two eminent surgeons of Kensington: the appearances which presented themselves were the following. On opening the thorax the right lung was found adhering firmly to the pleura, the heart perfectly healthy in structure, and of the natural size.

On the abdominal viscera being exposed, the liver was found much enlarged and indurated, a large quantity of coagulated blood in the cavity of the abdomen; upon minute examination the vena cava inferior was found extensively ruptured, in the situation of the fourth lumbar vertebra, just before the vein terminates by dividing into two iliac veins; the

coats were rather thinner than usual at the part that was ruptured, but there was no appearance of ulceration; the only symptom he complained of previous to the accident, was a pain in the right side. The subject of this accident was a robust young man, 24 years of age.—*Lancet*.

M. Roux, a distinguished surgeon in the department of the Var, has recently made an ingenious application of the principles of the Taliacotian operation to the treatment of cancers of the lips; borrowing from the lateral parts of the face and neck, the materials necessary to replace the loss of substance occasioned by their removal. His method is applicable to a great number of cases, and especially to those where the extent of ulceration prevents an incision in form of the letter V. Among the cases published by him, there is one, the principal circumstances of which are subjoined, in order to convey a better idea of the operation. The cancer occupied the lower lip, extending two inches towards the left cheek, an inch toward the right, and involved the whole chin. Five incisions were made by M. Roux, completely isolating the tumour; to remedy the great loss of substance occasioned by its removal, M. Roux called to mind the extension of which the skin is susceptible, and dissected freely the integuments of the neck throughout the whole extent of the inferior margin of the wound, prolonging the fifth incision downwards; this done, the head was bent forwards, inclining slightly towards the left side, and the chin covered by the portion of skin elevated by the dissection, so that the lower lip was replaced by the border of the third incision, which isolated the part of the tumour occupying the chin. Sutures and adhesive strips were applied, and the cure was completed by the twelfth day; no restraint being experienced in the movements of the head.

This operation, called *Ochylaphastique*, by M. Roux, is ingenious and rational, founded as it is upon one of the most evident properties of the skin; and we do not hesitate to consider it as one of the conquests of surgery, which will, ere long, be consecrated by general experience.—*Gazette de Santé*.

Examination of the Eyes of an Individual affected with Amaurosis; by Prof. Heusinger. —The subject had been amaurotic for a long time, but no particulars could be obtained in relation to his disease. The optic nerve on both sides was wasted; the globe of the eye was well formed, and possessed nearly its usual tension. Although the sclerotica was opened with much care, the escape, in considerable quantity, of a yellowish brown fluid, coagulable by alcohol, could not be prevented. The coagulum was of a beautiful pale yellow colour, which it retained for some hours. The choroid adhered strongly to the sclerotic coat, and appeared deficient in the black pigment.

The retina was contracted into folds, but presented no other unusual appearance; the vitreous body was almost entirely wanting; the crystalline was yellow and opaque; on the external surface of the retina, three pyriform, transparent vesicles were found, from the size of a pea to that of a small haricot; the contained fluid coagulated by alcohol, and is supposed by Prof. Heusinger, to have been of the same nature with that already mentioned, which had escaped from some of the vesicles being accidentally opened during the dissection. Both eyes were similarly affected. M. Heusinger is disposed to consider the vesicles as productions of the tunica Jacobi.—*Bull. Univ.*

Ectropium of the External Angle of the Eye.—This disease described by M. Walther, consisted in a reversion of the external angle of the eye, in consequence of a wound in the temporal region, attended with loss of substances. The palpebral commissure was rounded, distended, and had entirely lost its angular form; the conjunctiva had become inflamed, tumefied, and painful, occasioning great deformity. In proceeding to the operation, M. Walther removed, in the first instance, the cilia from the external part of the palpebrae, with a pair of forceps; he then seized the inferior lid at the point where the reversion commenced, and rendering it slightly tense, made an incision with a narrow, blunt-pointed bistoury, extending about a line beyond the commissure; a similar incision was made through the upper lid, the bleeding margins forming an acute angle externally, were brought together by means of sutures, and a complete cure was obtained.—*Bull. Univ. &c.*

New Method of distinguishing Barytes and Strontites.—To the mineral pulverized, a few drops of sulphuric acid, of 66°, are added; if strontites be the subject of experiment, there is a considerable disengagement of caloric; while, if it be barytes, this caloric is accompanied by light, the oxide becomes incandescent, and continues so for some time. It will frequently happen that the vessel in which the experiment is performed will be broken. It may be remarked, 1st, that if the experiment be made with barytes which has been long in powder, the incandescence and evolution of light does not take place, owing, doubtless, to the water which it has absorbed; 2d, the phenomena above mentioned, takes place also, when barytes not pulverized is made use of, but not so promptly. When quick-lime is employed, there is, as in the case of strontites, a disengagement of caloric.—M. Julia-Fontanelle, *Journal de Chem. Med.*

Separation of Bismuth from Mercury by Potassium.—M. Serullas has pointed a striking instance of the separation of bismuth from mercury. He says a twelve hundred thousandth, and even less of bismuth, when dissolved in mer-

cury, may be separated and rendered visible by the addition of a certain quantity of the amalgam of potassium and a little water. A black powder is observed to rise from the substance of the metal, and is a mixture of bismuth and mercury in a very divided state; it rises to the surface or adheres to the vessels.

Copper, lead, tin, and silver, are equally separated, but not so promptly, or so evidently to the eye as bismuth; for they are not associated with divided mercury, at the time of their separation, like the latter: with bismuth a mere atom is rendered visible, and M. Serullas thinks that chemistry does not present a more delicate test than the amalgam of potassium for bismuth in mercury.—*Annales de Chimie. Quarterly Journal.*

New Chloride of Manganese.—M. Dumas obtained this compound by putting a solution of manganic acid into contact with sulphuric acid and fused common salt. Water and the new chloride are formed; the former is retained by the acid, the latter volatilizes in a gaseous form, with a greenish tint, and when passed into a tube cooled to 5° or 4° of Fahrenheit, it condenses the liquid of a brownish-green colour. The most simple process appears to be to form a common green chameleon, to convert it into red chameleon by sulphuric acid, and to evaporate the solution, which will give a residue consisting of sulphate and manganate of potash. This mixture acted upon by concentrated sulphuric acid produces the solution of manganic acid, into which the common salt is to be thrown in small pieces until the vapours which rise are colourless; the latter effect is a sign that all the manganic acid is decomposed, and that muriatic acid only is produced. This chloride of manganese corresponds in proportions to the manganic acid; it is readily formed and examined, but not easily preserved. An analogous compound is obtained when a fluoride is used instead of the chloride, but a sufficient quantity for examination has not yet been procurable.—*Annales de Chimie.*

New Borate of Soda.—M. Payen lately presented to the Society of Pharmacy a new borate of soda, which will advantageously be substituted for calcined borax. It crystallizes in regular octahedrons, is harder than common borax, and is almost as sonorous as cast iron: its fracture is vitreous, and rather undulated. When immersed in water, the crystals become opaque, and retain their opacity in dry air.

This borate differs but little from common borax; except in containing less water of crystallization. It is more convenient for soldering copper than common borax, because it does not swell so much.—*Journal de Pharmacie.*

Elementary Nature of Bromine.—Iodine colours a solution of starch blue, bromine ren-

ders a similar solution orange colour. M. A. de la Rive added a few drops of bromine to a solution of starch coloured blue by iodine, and obtained a compound which gave two distinct colours with starch—one brown, the other yellow; the difference of colour corresponding with the two bromides of iodine described by M. Balard. These compounds of iodine and bromine, dissolved in a solution of starch, were subjected to the voltaic pile: immediately the yellow solution became blue about the negative pole and orange about the positive pole, indicating the separation and places of the iodine and bromine. Thus the smallest quantity of iodine may be discovered in bromine; but when the experiment was resorted to, to prove whether the idea thrown out, that bromine was a compound of chlorine and iodine, was founded in fact or not, it gave no such indication, and a solution of bromine in starch electrified for a long time together, gave no appearance of iodine. Hence M. de la Rive concludes, that bromine contains no iodine, but is an element analogous to iodine and chlorine. When bromine and iodine are combined, the former passes to the positive pole, and is consequently more negative than the latter; which accords with the observation of M. Balard, that it should occupy a place between chlorine and iodine. According to the *Bulleten Universel*, when the letter to M. Arago, containing an account of the facts above referred to, was read to the Academy of Sciences, that body decided that the assertion of M. Dumas, that bromine was a compound of chlorine and iodine, should be considered as retracted, and that it should be so entered upon the *proces-verbal* of the sitting.—*Quarterly Journal.*

On the Power of Water and Bromine in conducting Electricity.—M. de la Rive found, as had been previously ascertained by M. Balard, that pure dry bromine did not conduct the electricity of a voltaic battery, consisting of sixty pairs of plates very strongly charged, a delicate galvanometer being the test. A similar experiment was then made with pure water contained in a glass capsule, and communicated with the battery and galvanometer by platina wires; and the deviation of the needle was scarcely sensible. A few drops of bromine were then added to the water, which soon imparted a yellow colour to the water; being now included in the voltaic circuit, the galvanometer needle was deviated 70°, and an abundant disengagement of gas took place from the platina wires. There were oxygen and hydrogen in the usual proportions, showing that water only had been decomposed. From these experiments it results, that a body which conducts voltaic electricity very imperfectly, namely, pure water, may be rendered a good conductor by holding in solution a very minute quantity of a perfectly non-conducting substance, namely, bromine: the same fact occurs with iodine, and iodine and water.—*Annales de Chimie.*

Mercury detected in the Glands, &c.—Upon an analysis recently made in the Chemical Laboratory of the Faculty, this mineral has been obtained in its metallic state, from the mammary and salivary glands, and those of the mesentery and great intestine, taken from a young woman who died of puerperal peritonitis, for which it was largely employed in frictions, (twelve ounces and five drachms in the space of sixteen days.) This result, opposed to the assertion of a great number of chemists and physicians, is corroborative of the researches of MM. Fourcroy, Dumeril, Orfila and Cruveilhier, who have observed mercurial globules in the bones, and substance of the brain and nerves, and removes all doubt as to the correctness of the fact, notwithstanding the acrimony and ridicule with which it has been combated.—*Journal des Progrés, &c.*

Dr. Desaignes relates the following symptoms as arising from an attempt at suicide, with a drachm and a half of a solution of the hydriodate of potash. General *malaise*, nausea, and a sensation of burning heat, with acute pain in the neighbourhood of the epigastric region, immediately following the attempt. One hour afterwards, there supervened vomiting of a yellowish fluid, with a strongly marked taste of iodine, according to the patient; extreme agitation, continual moanings, paleness of the face, cephalalgia and vertigo; the two last symptoms continued after all the others had disappeared, and were not removed till the fifth day; there was neither fever nor gastric or intestinal inflammation.

The treatment was very simple, consisting in the administration of warm water to promote vomiting, enemata, a concentrated solution of gum as a ptisan, and some spoonfuls of an antispasmodic mixture.—*Journal de Chimie Med.*

Suffocation occasioned by the presence of a Leech in the Larynx.—The subject of this case, a soldier, swallowed a leech while drinking in a rivulet; a physician was called immediately and found him with his countenance suffused and tumefied, foaming at the mouth, and respiration almost entirely suspended; these symptoms ceased after a time, to appear again at various intervals. The presence of a foreign body in the larynx was suspected, and laryngotomy proposed as the only means of preserving life, but in the act of performing the operation, the unfortunate man expired in a state of suffocation. On opening the larynx, a leech was found adhering with such force to the right ventricle, that it could not be detached without difficulty. The same journal contains a case of hæmatemesis, arising from the presence of a leech in the stomach, which continued fifteen days, and ceased only by the discharge of the animal.—*Journal Universal, &c.*

Extirpation of the Uterus.—Dr. Blundell has recently performed one of the boldest operations in surgery, the complete extirpation

of the uterus per vaginam. The patient, upwards of forty years of age, was affected with cancer of the organ; she bore the operation, (necessarily tedious and painful,) with great fortitude—not more than six or eight ounces of blood were lost, and no ligature was applied. Up to the 16th day, not a single untoward symptom had occurred; there had been throughout little discharge from the part, and at the period mentioned, the wound had healed to within the extent of a shilling; the woman was free from pain, slept well, appetite good, &c.—*Lancet.*

Congenital Luxation of the Femora.—This phenomenon, for a detailed account of which we are indebted to M. Dupuytren, has recently fallen under the notice of M. Breschet, at the Hotel Dieu. The patient, an old man, sixty years of age, entered that institution for retention of urine. The bones were luxated backwards and upwards upon the ileum, occasioning a considerable projection of the glutei muscles; the knees were turned inwards; the patient was able to separate his extremities only to a very small degree,—as might be anticipated, his gait was very irregular. A more particular account of the case will be given by M. Breschet, in a memoir which he intends shortly to publish on the subject.—*Nouv. Biblioth. Médicale.*

Gangrene of the Uterus.—A woman, thirty-seven years of age, the mother of six children, had a prolapsus of the vagina, followed by an abundant leucorrhœal discharge after her last accouchment. Four years afterwards, she became pregnant the seventh time, during which she was attacked with fever, cough, oppression, &c. Towards the seventh month, dull uterine pains, the discharge of a greenish fluid, and other symptoms announced approaching abortion. Thirty-six hours, however, after the evacuation of the waters, the os uteri was observed very slightly dilated, and the uterus itself, appeared to have lost its expulsive power; the case was a foot presentation, and the fœtus was ultimately extracted with great difficulty, covered by a glutinous and putrid substance. The placenta was discharged spontaneously. The lochia did not appear, but the patient was attacked in quick succession with a severe shivering, cough, and an intense heat over the whole body; the pulse was small and frequent, and she was in a state of extreme anxiety; other symptoms subsequently developed themselves, pain in the thorax and right groin, a discharge of grumous matter from the uterus, vomiting, tension and pain in the abdomen, &c. &c. and terminated her existence five days after delivery.

On opening the abdomen, there was a discharge in considerable quantity of a milky fluid, which had deposited a concrete pellicle on the surface of the intestines, liver, &c.; it exhaled a fetid odour, and was not coagulable by the action of heat or vinegar. The liver

and spleen were voluminous, the heart large and flabby, weighing fourteen ounces and six scruples; the uterus was of a brown colour and possessed a gangrenous odour; purulent points were observed near the orifice of the tubes.—PALETTA. *Exercitationes Pathologicae, Jour. des Progrès.*

Successive Abolition of the Senses.—M. C. J——, a native of Corsica, and allied to the family of Napoleon, after a life of unrestrained indulgence, was suddenly attacked with gutta serena. Notwithstanding his blindness, he still continued to discharge the duties of his office with undiminished ability, his letters, papers, &c. being classified in such a manner, that he could find them without assistance in case of necessity. The other senses acquired a remarkable degree of perfection, and the touch became so delicate that he was able to recognise the different rare editions which he possessed, and could distinguish the manner in which the various engravings had been made; he played at trictrac, and was the delight of the society with which he associated. Some time later, his hearing became impaired, and complete deafness suddenly supervened. His memory continued in extraordinary vigour. At a subsequent period, general sensibility and contractility was extinguished; the whole surface of the body became insensible, and the extremities were gradually deprived of motion; still, however, the power of speech and mastication remained. His wife having discovered that one of his cheeks was slightly sensible, the active imagination of the patient, suggested to him the idea of having letters traced upon this part, which he pronounced as they were finished, and in this way could be made to comprehend several phrases. His mental faculties were so active, that he would frequently anticipate the meaning of the communication from the first syllable traced in the manner just mentioned; and this morbid energy of the intelligence increased in proportion as the faculties of relation were destroyed. His condition assumed progressively a more unfavourable aspect; the faces were passed involuntarily, and after several years of suffering, moral rather than physical, the unfortunate man expired. No examination was permitted.

During the development of his disease, M. J—— had a daughter, who was born an albino. M. de Fermon, the narrator of the case, inquires whether this condition is referrible to the alteration of the nervous system of the father.—*Bull. Univ.*

Periodical Contraction of the Extremities.—The patient, a robust young woman from the country, consulted Dr. Fallot in December 1826, and gave the following history of her case. Seven months previously, the catamenial discharge was suddenly suppressed in consequence of fright from a dog which she supposed to be rabid, and all the means subsequently employed to induce its recurrence

had proved ineffectual. In September she was troubled with an uneasy pricking sensation in the inferior extremities, accompanied with sudden and painful twitchings. In the following month, the contraction of the flexor muscles gradually proceeded to such an extent, that the heels were forcibly applied to the posterior part of the thighs, and every attempt at extension occasioned the most acute pain. At the expiration of five days, relaxation took place, and weakness of the affected limbs was the only remaining symptom.

The contraction recurred after the lapse of a month, and followed precisely the same course, leaving the gait more feeble and vacillating than before. In other respects, the patient appeared to be in perfect health.

Founding his treatment on the supposed existence of congestion of the spinal cord, its investing membranes, Dr. Fallot directed the application of thirty leeches from the loins to the coccyx; the flow of blood was considerable, but whether owing to its not being sufficiently so, or to the proximity of the paroxysm, it proved inadequate to prevent its recurrence, which took place three days afterwards, but not however to the same extent as formerly. The leeches were again applied the succeeding month and the contraction did not return; the prickling sensation and twitchings above mentioned, however, still continued, the latter not unlike those produced by electricity. The same treatment was continued at each subsequent period; the catamenia were re-established in April, and the patient thenceforth enjoyed her usual health.—*Journal Complementary, &c.*

Pills of Balsam of Copaiba.—M. Soubeiran read to the royal academy, a memoir relative to a new method employed by M. Mialhe of preparing pills from this article. His plan consists in mixing it with one-seventeenth its weight of calcined magnesia, which causes it to assume a proper consistence, after the lapse of a few days. This process furnishes also a mode of detecting the presence of castor oil, with which copaiba is frequently adulterated; the mixture in this case remaining soft and presenting an oleaginous appearance. *Archives Generales de Medecine.*

Remarkable lesion of the Rectum.—The posterior part of this intestine was perforated by the pipe of a syringe, and the fluid contained in the instrument, injected into the cavity of the pelvis. Violent symptoms immediately supervened, and a portion of the rectum became gangrenous, and was detached on the sixth day of the accident, leaving a large opening on its posterior surface, which afforded a passage for the contents of the bowel, and copious injections of emollient fluids were necessary for their dislodgment. Professor Graefe, who was consulted in the case, directed a vegetable diet, pills composed of galbanum and myrrh, injections, consisting of Venice turpentine, diffused in chamomile

water, by means of the yolk of an egg; and lastly, the introduction into the rectum, of the intestine of an animal, distended with water, for which air was afterwards substituted. The preceding treatment was continued about two months, during which the accidental opening gradually diminished, and a complete cure was ultimately obtained without the slightest contraction.—*Archives Generales de Med.*

Spasmodic Contraction of the Urethra.—The following case is extracted from an inaugural dissertation by Dr. Reimoneng of Montpellier, as presenting a remarkable example of this species of stricture. A young man, after violent mental agitation, was attacked with complete retention of urine, and when Dr. R. saw him, the bladder was greatly distended, reaching as high as the umbilicus; the hypogastrium was very tender to the touch, and the patient in a state of agitation little short of delirium. Ineffectual attempts were made to introduce the catheter through the stricture, which appeared elastic and yielding before the point of the instrument,—a considerable hemorrhage followed the operation. Some time after, by dint of great exertion, half a pint of urine was evacuated, which was productive of slight relief; he was bled to the extent of twenty ounces, thirty leeches were directed to the perineum, emollient fomentations, enemata, the warm bath, &c. were successively resorted to, but no further evacuation could be procured. Subsequent attempts to pass the catheter, cat-gut bougie, silver sounds, &c. were as unsuccessful as on the former occasion; the obstruction appeared to be situated about six inches from the orifice of the urethra. A grain of opium was given in the evening which procured some rest; during the night, the patient was awakened by a desire to pass his urine, which now flowed in a full stream, and when Dr. R. repeated his visit in the morning, a bougie No. 6, could be introduced into the bladder without difficulty; he then learnt that his patient had suffered an analogous attack, two years before, brought on by a similar cause, and that it had yielded to a warm bath, continued for two hours, during part of which time he slept.—*Journal des Progrés, &c.*

Expulsion of the Placenta after Abortion.—Dr. Prost, of Vienna, relates a case in which the placenta was expelled eight months after miscarriage had taken place. In another instance the placenta did not come away until one hundred and three days after abortion. Mauriceau, Morgagni, Bonnet, &c., have recorded similar cases. Prost infers, then, that it is more judicious to leave the separation of the placenta to nature, than to use any violent means for its removal during the early months of pregnancy, when abortion takes place. If, however, considerable hemorrhage follow the miscarriage, it may be inferred that the placenta is separated from the uterus, and then it may in most cases be removed without difficulty.—*Lon. Med. Gazette.*

Structure of the Nerves.—MM. Breschet and Raspail have lately been engaged in investigating the structure of the nerves, with a view to ascertain whether there were canals, according to the opinions of the late M. Bogros, permeating their centre or not? The conclusion at which these anatomists have arrived is, that there are no canals in the centre of the nerves, and consequently that it is not by virtue of any fluid contained in them, that the powers of sensation and voluntary motion are performed. In all the cases where an attempt was made to inject mercury into the nerve, this fluid either escaped between the loose cellular membrane and the neurilma, or the neurilma and the nerves; and in no instance continuously through the centre of the nerve.—*Repertoire de Anat.*

Sabulous Formation in the Brain.—Dr. Bergmann of Celle, in a memoir transmitted to the Royal Society of Gottengen, gives an account of twenty cases of earthy granulations occurring in the plexus choroides of the lateral ventricles of the brains of insane persons. These earthy granulations resemble those of the pineal gland. Mr. Stromeyer examined both varieties, and found their constitution similar, and of the following nature: Phosphat of lime in large quantity, phosphat of magnesia in small proportion, traces of carbonate of lime, and an animal substance of an albuminous nature.—*Bullet. Univ. c. x. p. 128.*

Crystallization of Metallic Oxides.—If a solution of nitrate of copper, mingled with very fine charcoal powder, or even deutoxide of copper, be put into a similar tube to that described in the last article, then a plate of copper be introduced and the vessel closed up, in about fifteen days small red transparent octoedral crystals of protoxide of copper will be formed on the plate of metal. Other metals have been subjected to similar experiments, but probably have not yet remained long enough under action.—*Ann. de Chimie.*

Dr. Bally has lately employed strychnine internally in paralysis of the bladder, and has obtained from its use very decided benefit. Three patients have been cured in less than a month by this remedy, given in doses from one-tenth of a grain to two grains, every four and twenty hours.—*Lancet.*

Urinary Calculi in a singular situation.—One hundred and six calculi, of the second class of Dr. Wollaston, were taken from beneath the prepuce of a man labouring under phymosis! The contraction of the urethra had led to the admission of the urine between the glans penis and prepuce, where the fluid had undergone a sort of spontaneous analysis, or decomposition.—(*Memorie dell' Instituto Lombardo-Venetiano.*)

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